# THE ANNALS

OF

# ELECTRICITY, MAGNETISM, & CHEMISTRY;

AND

# Guardian of Experimental Science.

CONDUCTED BY


WILLIAM STURGEON,

Lecturer on Experimental Philosophy, at the Honourable East India Company's Military Seminary, Addiscombe, &c. &c.


AND

ASSISTED BY GENTLEMEN EMINENT IN THESE DEPARTMENTS OF PHILOSOPHY.


VOL. III.—JULY, 1838, TO APRIL, 1839.

London:

Published by Sherwood, Gilbert, and Piper, Paternoster Row; and W. Annan, 12, Gracechurch Street.

Sold also by Messrs. Hodges and Smith, and Fannin and Co. Dublin; Maclachlan and Stewart, and Carfrae and Son, Edinburgh; Mr. Robertson, Glasgow; Mr. Smith, Aberdeen; and Mr. Dobson, No. 108, Chestnut-Street, Philadelphia.

1839.

From the
Family of
Prof. Joseph Lovering

## INDEX TO VOL. III.

# ERRATA.

From page 165 the pages are numbered 100 too high.

Page 111—line 29, for "sulphuric acid" *read* "nitro-sulphuric acid."

112—line 23, for "is settled" *read* "becomes established."

—line 24, after "that" *introduce* "means."

113—line 7, for "pile" *read* "pair."

—line 9, for "beneath" *read* "dissolved."

—line 35, for "often varies" *read* "varies according to."

114—line 10, for "discovered" *read* "exposed."

—line 12, after "potassa" *introduce* "in distilled water in the proportion of 1 hydriodate to 25 or 50 of the water. I now caused the current of a single pair to traverse two portions of the solution."

—line 19, for "minutes" *read* "seconds."

—line 20, for "foam" *read* "stratum."

116—line 14, for "any" *read* "one."

—line 31, *dele* "it" and "often."

—line 32, for "time" *read* "length."

—line 32, for "settling of the liquid" *read* "liquid stratum."

—line 40, for "calculated" *read* "transmitted."

117—line 30, for "proper" *read* "due" and for "of the" *read* "to."

—line 31, for "of metals" *read* "to the metals."

118—line 16, for "thread" *read* "wire."

—line 17, for "equal" *read* "constant."

—line 31, for "the principal" *read* "a" and *dele* "used in cases.

—line 35, for "this" *read* "such."

119—line 16, for "action" *read* "process."

THE ANNALS

OF

*ELECTRICITY, MAGNETISM,*

*AND CHEMISTRY;*

AND

**Guardian of Experimental Science.**

*JULY*, 1838.

I. *On the effects of the iron used in constructing ships (especially of that in iron steam vessels) upon the mariner's compass. By* C. F. Walker, Esq.

Read before the London Electrical Society, June 2, 1838.

1. Being well aware that the great inducement to cultivate the kindred sciences of Electricity and Magnetism is the hope of rendering them practically useful to mankind, I have collated some observations on the influence exerted on the magnetic needle by the combined action of the metal, dispersed in various parts of ships. I have been led to this from hearing of the great amount of disturbing force produced from the metal of an iron steam ship; and in the hope, that, from the pen of some of our talented members, we may receive some suggestions as to the best methods either of *counteracting* it, or of *allowing* for it.

2. I have been given to understand that a vessel of this kind is hailed as a great triumph in the era of ship-building; and that such ships would come into very general use, were it not for the effects on the needle. And, unless some simple plan be suggested to obviate these difficulties, I see not how we can derive any further service from them than as mere river boats.

3. In a nation like England, where commerce is the very nerve and sinews of her existence, our best energies are called for to facilitate her means of carrying it on; and though *we* are not expected to decide as to the relative merit of this or that mode of ship-building; yet, as the Electrical Society of the first commercial city in the world, we are bound each to contribute his mite toward rendering available any mode which they (who are skilled in these matters) may suggest.

4. The effect of the ship's iron on the needle had long been the hidden cause of many disastrous shipwrecks, which were at one time attributed to some strong current; at another time to the mistaking of one headland for another; or, in fact, to any of those common-place causes so readily framed to account for an accident after it has happened. And even when the attention of nautical men was led, or, I may say, forced, to acknowledge that something affected the needle, yet the true cause long remained concealed. Yes! And though some strange anomalies had occurred to Mr. Wales with regard to the direction of the needle, when either its position or the ship's course was altered, yet neither he nor the investigating Capt. Cook (with whom he sailed as astronomer) succeeded in detecting the cause.

5. We first find light thrown on this subject about the year 1790, when Mr. Downie, master of H. M. S. Glory, reports thus: "I am convinced that the quantity and vicinity of iron in most ships have an effect in attracting the needle; for it is found by experience that the needle will not always point in the same direction, when placed in different parts of the ship. Also, it is rarely found that two ships steering in the same course by their respective compasses will go exactly parallel to each other.*

6. But we are indebted to Capt. Flinders, in 1801 and 1802, of H. M. S. Investigator, for having pursued the enquiry, and established this general truth, that, "when the ship's head was on the *east side* of the meridian, the differences were mostly *one* way; and when on the *west side* they were the *contrary:* whence he judged that the iron of the ship had an attraction on the needle and drew it *forward.* But there was this remarkable distinction: in the northern hemisphere, it was the north end of the needle which was attracted; and in the southern hemisphere, it was the south end."†

7. This will be better understood by referring to the diagrams.

Fig. 1 Plate I. represents a vessel in the northern hemisphere, sailing eastward, with the north pole of its needle, N S, deflected eastward, or toward the head. Fig. 2 is the same pursuing a westward course, having the north

* Copied from the work on Magnetism of the Society for the diffusion of Useful Knowledge (§ 245); there quoted from Walker's Treatise on Magnetism.

† Copied from Bain's Essay on the Variation of the Compass. (p. 65, 66.)

pole of its needle, N S, deflected west, or toward the head, as before.

Figs. 3 and 4 represent vessels in the southern hemisphere where the north end of the needle deflects toward the head of the vessel. In all, the lines, $x$ $y$, represent the magnetic meridian. The line, W E, is the magnetic equator. I would here observe, that, wherever these words *meridian* and *equator* occur, they are, in the present paper, used in reference to the *magnetic* meridian and equator.

8. It is unnecessary to relate the many observations since made tending to confirm this. They will be found abundantly in Bain's Essay, above alluded to, and in Barlow's work on Magnetic attractions.

9. The cause of these deviations will be readily manifest to all who are acquainted with the laws of magnetic action. For example.—Conceive a vessel to be pursuing an easterly course on the north side of the equator, as Fig. 1, it is required from the known laws of magnetic action to determine the effect of the iron it contains on the compass.

10. It is well known that if a bar of soft iron be held in the direction of the dipping needle (which direction at London makes an angle of about 70° with the horizon, as in Fig. 5), the inductive influence of the earth will convert it into a temporary magnet: and, if the experiment be performed in our hemisphere, namely, north of the equator, the upper end of the bar will be a south pole, and the lower a north. The theory of this is as follows.—

11. Since opposite poles attract, the end of *our* needle which is directed towards the *north* must be drawn thither by a force corresponding to a *south* pole; which south pole, therefore, will produce north polarity at N. (Fig. 5) the lower end of the bar, and south polarity at S, the upper end. Nor is it requisite that the bar should be of a regular form: the same effects will be produced on any piece of iron, to a greater or less extent, according to its *shape, size*, and *situation*: and hence every distinct piece of iron in the ship becomes a magnet, with its north pole upwards.

12. But, since the binnacle is placed very far aft, it follows that almost the whole of these temporary magnets are between it and the head of the vessel, and as they have each their north pole upwards, the united force of all inevitably tends to draw the *north* end of the needle toward the head, as is shown in Figs. 1 and 2. If we make our observations in the southern hemisphere, the reverse is the case, as is seen in figures 3 and 4.

13. But, when we attempt to decide on the *degree* of deviation thus produced, theory fails; because it cannot possibly obtain the requisite data.

The bolts, water-tanks, guns, shot, stores, and engines, all according to their *size*, *positions*, and *distances* (and, I may add, hardness or softness of their metals), must be taken into any mathematical calculation: and even were it possible to do this, the conclusions would but be correct for smooth water. For every time the ship pitched or rolled, the power of the centre of attraction would change; because, as above stated, the magnetic condition of iron depends on its *position*, as well as on its *size* and *distance* from the needle. Each time, therefore, it changed its position, with respect to the direction of the dipping needle, it would alter in its magnetic condition.

14. This may be easily illustrated by placing the upper end of a piece of soft iron within some certain distance from a compass needle and then changing its position (without altering the distance of the upper end), till it coincides with the dip; it will be observed that as it *approaches* that coincidence, the deviation will *increase*, and as it *recedes* it will *decrease*. This irregularity, therefore, we cannot expect to obviate.

15. But, passing on, another difficulty occurs. Captain Flinders observed that the variation differs in different latitudes, that the quantities were greater in high and less in low latitudes, as we may observe from the annexed extract from observations made by him on board H. M. S., Investigator.

TABLE I.

| Lat. S. | Long W. | Ship's Head, E. b. N. | | Variation W. |
|---|---|---|---|---|
| 34° 16′ | 137° 16′ | " | " | 0° 10′ |
| 32 40 | 125 20 | " | " | 7 25 |
| 32 7 | 128 1 | " | " | 6 4 |
| 32 15 | 132 39 | " | " | 2 49* |

16. He observes that the quantity of variation did not increase and diminish in proportion to the latitude; but soon found that the errors had a close connexion with the *dip of the needle;* and that the connexion between them was so far certain as to make the *dip* our datum in reducing the observed to the true variation. (Bain's Essay, p. 114.)

17. But, as Mr. Barlow observes in his preface, "this datum is not easily obtained; for every one who is acquainted

* Copied from Bain's Essay, page 67.

with the nature of the dipping needle must be well aware, that it is much too delicate ever to become an efficacious sea instrument."* So that, unless we are able to solve the problem of latitude and longitude being given to find the dip, an ingenious rule, which Captain Flinders based on the being able to obtain this datum, is not available. This induced Mr. Barlow to perform a tedious series of experiments in order to determine the effects of iron, at different distances, and in different positions.

18. Having prepared a strong table, he divided its plain accurately into equal parts, by lines from the centre to the circumference. A hole was then cut in the centre, over which was suspended an iron ball weighing 288 pounds. By means of pulleys he could raise or lower this as he pleased. Placing it now at one elevation, now at another; and observing, at each position, its effects on the needle placed on each of the radii drawn on the table, he arrives at the following conclusions.

19. If the position of the dipping needle represent the axis of the ball, a plane perpendicular to this axis, passing through its centre, would be a circle of no variation: that is to say, if the centre of a needle coincided with this plane, no variation would be found to take place. The plane, at right angles to this, is also a plane of no variation. The amount of variation depends on the latitude and longitude of the needle with respect to these planes; the *former* of which represents the equator of the ball; the *latter* its first meridian.

20. To apply this to irregular masses of iron, he was induced to perform a similar series of experiments, wherein he substituted, for the ball above mentioned, a twenty-four pounder cannon, weighing 58 hundred weight; and was greatly gratified to find, as he had anticipated, that the plane of no attraction exists in the most irregular masses; and, therefore, exists in ships which contain many of these masses.

21. It then occurred to him, that, if a large mass of metal, A, fig. 6, at distance A B, produced a certain deviation, a smaller mass, C, at distance C B, would produce the same; and that if C were in the line which joined A and B, the tangents of B's (the needle's) deviation would be doubled. But, as the tangents of *small* arcs are nearly equal to the arcs themselves, we shall not be far from the truth if we consider the arcs themselves as being doubled.

22. The impossibility of discovering by calculation the place of A, that is, the direction of the resultant line produced

* Preface to Barlow's Essay, p. 6.

from the composition of all those forces emanating from each piece of iron in the vessel, will be at once perceived, from what has been mentioned above (§ 13). It was, therefore, requisite to warp the vessel round to each point of the compass, and to observe what effect the ship's iron had on the needle in the different positions; upon the same principle as that which regulated the experiments on the iron ball and on the gun; and so to determine the planes of no variation, and hence the direction of the required resultant line, A, B. Having found this, it was next requisite to determine on *shore* (by repeated trials) at what distance the smaller mass, C, should be placed, in order to produce the same amount of deviation as was produced by the iron in the vessel. And having obtained this, the mass C was to be placed in the line A B, at the determined distance B C, and adjusted so that the line might pass through the pivot of the needle and the centres of attraction of both A and C. It was then removed and only used to correct observations, as follows :—

23. Let the compass indicate that the vessel's head is 5° N. of E.; then place C in the line of direction, at the previously determined distance; and, if the compass should declare the vessel's head to be 8° N. of E., the additional 3° in the amount will be due to C. But, since C has the same effect as A, the deviation produced by A is 3°; deduct this from 5°, then the remaining 2° N. of E. is the ship's true position. It will be right to remark here, that Mr. Barlow discovered the attractive force of iron to depend not so much on its mass as on its surface; and, hence he employed two pieces of sheet iron, separated by a thin board, as the compensating plate C. I am not informed whether these suggestions have been adopted either in merchant vessels or in Her Majesty's Navy. They certainly bear the stamp of deep research, and will ever reflect great credit on their author, from the time and talent he employed in their investigation; especially too when it is remembered that he expected individually to reap no other reward from his labours than the pleasure of having contributed his portion toward the improvement of an important science. We must also bear in mind that he did not publish them, as being *without* objection: he saw that difficulties still existed, although this one step was gained.

24. In referring again to fig. 6, I am not prepared to say whether theory warrants us in concluding that the resultant A B is constant. But, since a change in the direction of the dip effects a like change in the magnetic *condition* of each element producing that resultant, the ques-

tion arises, will not its *direction* be in like manner *changed?* Experiment alone can *satisfactorily* solve this problem.

25. Yet, granting it to be constant, it is, I should imagine, no easy matter to *determine* the direction of this line, and also the place of the compensating plate C; and, should an error occur in either or both of these, all conclusions would be dangerously erroneous.

26. I have thus as briefly as possible endeavoured to trace the progress hitherto made; and, if this statement does but display difficulties besetting every step, it is laid before this Society in the hope of its meeting with the attention so important a subject deserves, from those members who are far more able than myself to solve them.

27. I have now to read, on the same subject, some original observations made on board the Rainbow, an iron steam vessel, the property of the General Steam Navigation Company, which lately arrived at the port of London from Liverpool. I must premise that the Captain who navigated it was not aware of the matters being brought before this Society, and that the observations are extracted from his note book, roughly entered as they were noticed. This will be a sufficient apology for their paucity. The subject was, in a like casual manner, mentioned to myself; and it occurred that these notes, few as they are, and for which, through the kindness of a friend, I am indebted to the Captain, would not be uninteresting to this Society. [See note 1 appendix.]

28. It will be here observed, not only that the deviation is very considerable, as much as 56°, but that it is the reverse of what occurs in all other cases. By referring to figures 1 and 2, the compass deviates toward the *head* of the vessel; but in the present instance it deviates toward the *stern*. This induced me to write to the Captain in order to obtain a confirmation of his former notes, and to propose the following questions which, with the answers received, I will now read. [See note 2 and 3 appendix.]

29. It will be here observed, that although 8-9ths of the vessel, (containing the engine and boiler weighing 140 tons), is in front of the binnacle, (which is placed midships), yet the needle is drawn toward the *stern;* and that even when the compass is elevated, though the *deviation* decreases in amount, yet the *direction* remains the same. This can be accounted for only on the supposition, that, as in the former cases, (the cases of wooden built ships) the united actions of *many* magnets affected the needle: in the present, it is affected by the *alone* action of the iron vessel, which becomes one *large*

magnet having its *stern* a south pole, *when the above observations were made.*

30. My information does not define precisely the then position of the ship, so as to enable my giving a diagram; but a little reflection will suggest, that, should the vessel's *head* be towards the north, the *stern* would be a south pole, and vice versâ.

31. The very recent date of building iron vessels, and consequently the want of experiment to supply sufficient data, prevents the offering many suggestions as to the *theory* of their action. If we turn to enquire how this action can be obviated or counteracted, it is readily seen that we are venturing on hitherto untrodden ground. For, though the compensating plate of Barlow may be available when the arc of deviation is small, (as on board other vessels), yet here, on account of the much larger one, it is evidently impracticable.

TABLE II.

| Ship. | Commander. | Place. | Local attraction. |
|---|---|---|---|
| Conway | Capt. Basil Hall | Portsmouth | 4° 32′ |
| Leven | Capt. Owen | Northfleet | 6° 7′ |
| Barracouta | Capt. Cutfield | " | 14° 30′ |
| Hecla | Capt. Sir E. Parry | " | 7° 27′ |
| Fury | Capt. Hopner | " | 6° 22′ |
| Griper | Capt. Clavering | Nore | 13° 36′ |
| Adventure | Capt. King | Plymouth | 7° 48′ |
| Gloucester | Capt. Stuart | Channel | 9° 30′ |
| | | 8 | 69° 52′ |
| | | Mean | 8° 44′ |

32. By referring to the above table of deviations in different ships, copied from Dr. Roget's work (p. 259), it will be seen that the mean deviation is about 8½°. Twice the tangent to *this* arc is about equal to twice the arc itself; and, therefore, when by the application of the plate C (fig. 6), the former is doubled, but a *small* error is committed when the latter is considered as doubled also. But twice the tangent of a *large* angle is by no means equal to twice the arc: and hence this method is objectionable where the deviation amounts to so many degrees as that on board the iron vessel.*

* This may readily be seen on consulting any table of tangents and arcs.

33. Barlow has proposed the following formula where great accuracy is required,

$$\tan. x = \frac{1 + \sqrt{1 - 8 \tan.^2 a}}{4 \tan. a}$$

where $x$ is the angle of deviation produced by the *ship*, and $a$ that produced by the *plate*.

This is an accurate method; but to the difficulties above mentioned of finding the resultant line A B, and the distance B C of the plate C, is added the having to solve a mathematical formula. How far, therefore, this theory is practicably available remains to be proved by careful experiment on board the vessels themselves.

34. In conclusion, I would offer *two* suggestions to the owners of those vessels, not having observed them in any work I have consulted on the subject.

First. To select some elevated spot for a *standard* compass.

Second. To steer on observed (not on true) courses.

I. By elevating the compass only *ten* feet, and removing it in a favourable direction only *four* feet, the attraction which was from 4 to 5 points is reduced from 1 to 2 points. Could not some place be selected at *more* than ten feet from the deck, and at *more* than sixteen feet from the stern, where a compass could be placed whenever it was required to ascertain the true magnetic meridian? And could not the binnacle compass be corrected from this?

II. The second suggestion is this. Suppose the ship be to trade between London and Hamburg. At any convenient place in the Thames, warp the head round to each of the 32 points in succession, observing its direction as shown by the binnacle compass, whilst a coadjutor on shore makes the same observations from some conspicuous station. When this is done, let notes be compared, and a table drawn out of the angle of deviation in each of the 32 positions. Again, since the difference of latitude and longitude between London and Hamburg causes a difference in the dip, let a similar table be prepared when the ship is there, and perhaps at one or more intermediate places, at the discretion of the practised navigator. Let the vessel be steered on these observed courses in the following manner.

Annexed is a table of errors in the Isabella's compass at Shetland, observed by Captain Sabine, which may conveniently represent a table obtained as above, (in fact it is obtained as above).

## TABLE III.

| Direction of ship's head. | Deviations. | Direction of ship's head. | Deviations. |
|---|---|---|---|
| North. | + 1° 26′ | North. | + 1° 26′ |
| N. by E. | + 0 26 | N. by W. | + 2 26 |
| N. N. E. | — 0 19 | N. N. W. | + 3 26 |
| N. E. by N. | — 1 19 | N. W. by N. | + 4 26 |
| N. E. | — 2 9 | N. W. | + 5 11 |
| N. E. by E. | — 3 4 | N. W. by W. | + 5 46 |
| E. N. E. | — 3 34 | W. N. W. | + 5 46 |
| E. by N. | — 4 4 | W. by N. | + 5 41 |
| East. | — 4 34 | West. | + 5 11 |
| E. by S. | — 5 34 | W. by S. | + 4 11 |
| E. S. E. | — 5 34 | W. S. W. | + 3 56 |
| S. E. by E. | — 5 34 | S. W. by W. | + 2 56 |
| S. E. | — 4 59 | S. W. | + 1 11 |
| S. E. by S. | — 4 24 | S. W. by S. | + 0 26 |
| S. S. E. | — 3 34 | S. S. W. | — 0 0 |
| S. by E. | — 3 4 | S. by W. | — 1 34 |
| South. | — 2 4 | South. | — 2 4 |

It is required to steer N. E. by E.

On consulting the table, it is seen that when the ship's head is in that direction, the deviation caused by local attraction is 3° 4′, that is to say, 3° 4′ to the *west*, or *left side* of the meridian. If, then, the ship be steered 3° 4′ to the left of N. E. by E. by the compass, the desired end will be obtained.

If it is required to steer east, the course by the compass will be (on again consulting the table,) 4° 34′ to the left of east. If E. by S. 5° 34′, to the left of that point, and so of the rest.

35. Should this method be found available in practice, as in theory, it carries in its favour the one great recommendation of simplicity. It requires but a single class of observations; and these, too, within the reach of any practised navigator. It needs no mathematical calculations. And, lastly, it dispenses with the solution of any trigonometrical formula, often difficult, always annoying.

36. In addition, it is greatly desirable that the owners of such vessels should instruct their captains to make a careful register of any phenomena that fall within their sphere of observation, in order that the philosophical enquirer may have more copious data whereon to base his opinions.

The fruits of such labours will, I am sure, always be kindly received and readily welcomed by this Society. And, in the mean time, should their avocations or pleasures call any of

our members to make a voyage on board an iron Steam Ship, it is to be hoped that they will return laden with their quota of observations to add to that mass which must of necessity be accumulated ere the full elucidation of this important subject can be obtained.

The field of enquiry is in a manner new; the fallow ground is scarcely broken open; and a rich harvest—even the high honour of having contributed to the commercial prosperity of his country—awaits the successful labourer.

*Kennington Grammar School,*
*June* 1, 1838.

---

## APPENDIX.

### *Note* 1.

With the ship's head to the westward, and with the compass elevated *ten* feet above the deck, there was from one to two points attraction to the eastward; and with the compass in the binnacle on deck there was from four to five points easterly attraction. With the ship's head to the eastward there was as much attraction to the westward.

### *Note* 2.—*Questions proposed.*

1. About what distance from the head and from the stern is the binnacle placed?
2. Is the binnacle midway between the ship's sides?
3. What distance is the engine from the binnacle, and what is the weight on a rough calculation?
4. When the vessel is on the magnetic meridian, or nearly so, is the needle correct? If not what is the error?

4½. Is the needle equally affected whether the vessel's head be directed to the north or to the south?

5. How and where was the compass elevated ten feet?
6. What is the weight of iron in the vessel exclusive of the machinery?
7. When the vessel rolls, what is the effect?

### *Note* 3.—*Answers to the Questions proposed through* Mr. Hammond.

1. The binnacle is placed about 170 feet from the ship's head, and about 12 feet from the stern.
2. The binnacle is midway between the ship's sides.
3. The engine is about 80 feet from the binnacle; the weight of the engine with the boiler is, on a rough calculation, about 140 tons.

4. See Note at the end of the Appendix.
5. The elevated compass was placed about 16 feet from the stern of the ship.
6. The weight of iron in the ship I can form no idea of, the vessel being built of iron, except the beams, decks, and joiners' work.
7. There is no additional effect on the compass when the vessel rolls.

*A note from the Captain.*

Sir,

In reply to yours of the 19th iust. I herewith hand you the answer to the former questions. I am sorry to inform you that I had no fair opportunity of proving the compasses upon a north or south course to be able to report upon.

When the ship's head was to the eastward, the north point of the compass is attracted to the west from 1 to 1½ points; and with the ship's head to the westward, the north point of the compass is attracted to eastward from 1 to 1½ points.

---

## II. *On Electro-magnetic coil Machines. In a letter to the Editor, from* Mr. Uriah Clarke.

Leicester, June, 1838.

Sir,

Persuaded that you feel an interest in everything connected with the sciences of electricity and magnetism, and more especially with anything that may lead to an explication of the phenomena of nature, I take the liberty of communicating to you (for insertion in your admirable Annals, or otherwise, as you may think well) an experiment which I have made, and which is, I think, calculated to support the opinion of some recent theorists, that the earth acquires and maintains its polarity by galvanic agency; to effect this experiment a globular battery is first constructed in the following manner: by passing right through the centre of a hollow copper sphere of four or five inches diameter, a tube of zinc of one inch diameter, in which tube of zinc is placed a bar of soft iron having upon it a helical coil of covered copper wire, one end of which wire is soldered to the zinc tube and the other end to the copper globe. Care must be taken that the zinc and copper do not touch each other, but the joining must be made good by sealingwax or some other non-conducting substance; a saturated solution of common salt is then to be poured into the globe, and the globe suspended by means of an agate cap or floated in a vessel of water, and it will immediately indicate

polarity, arranging itself in the magnetic meridian. Pour out the salt water, and the effect of course ceases. If we may assume that there is any analogy between the action of the salt water on the metals of this little globe and that of the waters of the ocean upon the metallic strata of the earth, are we not at once furnished with a grand final cause for the saltness, as well as the vastness, of the waters of the sea?

I also subjoin a description of an electro-magnetic machine which I have contrived, and which I humbly think possesses an advantage in point of appearance over any I have yet seen or heard of. The rather common form of the bobbin or reel as usually constructed is here exchanged for the classic elegance of a pedestal, as seen in fig. 7, plate I., the upper part or capital of which, lifts off in the manner of a lid to admit the bundle of insulated wires. The terminations of the primary and secondary coils pass through the base of the pedestal as well as through the platform on which it is mounted, and are joined underneath to the respective pillars. To the brass studs 1 and 2 are fastened the ends of the secondary coil, and the conductors are attached by means of binding screws for physiological purposes; the terminations of the primary coil are joined to the pillars 3 and 4 as shown at the end view of the machine fig. 8. Where the battery communication is made, the break is effected by means of a brass pulley, having in it wood pegs, the contrivance of Mr. Nesbit, and which answers extremely well with the occasional use of a bit of sand paper, though I find a similar pulley of steel having in it two grooves, one milled and the other plain, to answer, I think, as well. The quantity of wire upon my coils is 40 yards of one sixteenth of an inch thick for the primary, and about 300 yards of very thin for the secondary coil, and when charged by a small sustaining battery completely fastens the hands to the conductors.

I am, Sir,
Very respectfully yours,
URIAH CLARKE.

---

## III. *On the Amalgamation of potassium by Voltaic Action. In a letter to the Editor, from* T. Mason, Jun. Esq.

Sir,

A few weeks ago I was induced to try the experiment of making an amalgam of mercury and potassium with a small

galvanic battery of twelve pairs, and having succeeded far beyond my anticipation, I send an account of the means employed. As it forms a beautiful and striking experiment, many of the readers of your valuable journal may be induced to repeat it.

To the zinc end of the battery I attached a platinum pole, which I connected with one ounce of mercury in a watch glass; to the copper, a wire with a piece of charcoal. I then put some hydrate of potash in small pieces on the mercury, and placed the charcoal on each piece till it was decomposed. In about ten minutes I poured the amalgam into an evaporating dish, and it became solid as soon as cold. It is hard enough to be handled and retains its shape in water for a considerable time, decomposing it and giving out hydrogen, which continues for more than an hour. The battery is composed of twelve half pint pots. A circular copper is fitted to the interior, to which is attached a mercury cup. The zinc is folded in a circular piece of brown paper, and placed in the centre, the wire dipping into the mercury cup of the next copper. The action of the battery when brown paper is used, instead of bladder, I find nearly doubled The jar is about half filled with a solution of sulphate of copper, and the zinc when put in its place filled with a strong solution of salt and water. One of the jars and its metals are shown in fig. 19, plate II.

After several hours' action no copper can be discovered in the solution of salt. One great convenience of this form of battery is the facility with which the zincs can be cleaned.

I remain,
Yours very respectfully,
T. MASON, Jun.

70, *High Holborn,*
*June* 16, 1838.

---

## IV. *On an Electro-magnetic Engine. In a Letter to the Editor, from the* Rev. F. Lockey.

Sir,

In consequence of the invitation on the cover of your last number, that individuals who have interested themselves in the construction of electro-magnetic machines should transmit for insertion in your interesting and useful "Annals," an account of their labours, at least so far as they develop any originality of arrangement, I am induced, though perhaps somewhat prematurely, to make my present communication. I say "perhaps prematurely" because my apparatus, though

commenced in December last, still remains incomplete; a variety of causes, professional and domestic, having combined to interrupt my progress in constructing it. Of the actual power of my arrangement I can therefore as yet say nothing.

Desiring to be brief I shall not trouble you with the merely mechanical details of my machine, but chiefly state that which (so far as I am aware) is peculiar to it. I have arranged the wire covered iron bars somewhat after the manner of a cheval de frise. The accompanying sketch, fig. 9, plate I. will probably convey my idea intelligibly,

Had I used horse-shoe bars, both for the fixed and moveable sets of magnets, advantage would have been gained in the machine occupying rather less space; but as the difficulty of covering such bars with wire is greater, and as this machine is designed merely to test the above peculiarity of arrangement (which we may call the *compound radii*) I determined to use straight bars. The horse-shoe form would also have brought the whole excited power into action, whereas, now, the magnetism of one end of each bar, moveable as well as fixed, is inoperative. It might also be advantageous to make the outer portion of the arrangement moveable instead of the interior, as in my apparatus. The size of my model is a diameter of 20 inches, the central moveable wheel or barrel bearing the compound radii 12 inches, the bars 4 inches long.

I have adopted only eight radii. Only three of the outer and fixed sets of bars are shown in the accompanying figure, in order to avoid confusion; the places of the remainder are indicated by the dotted outlines. They are set with the extremest regard to precision and symmetry; the polarity of each set of bars in the barrel being reversed at the moment of its coming into parallelism with the outer and fixed bars. Sixteen compound radii would promise more effectual action. Having but eight, the working poles are now about 4 inches apart. Half that distance would be far more effective, since the attractive and repulsive power of magnetism decreases in so large a ratio to the increase of distance between the magnetized surfaces. In all the plans I have hitherto seen proposed, it has been necessary to construct a commutator competent to reverse the polarity of the revolving bars as many times in every revolution, as there have been bars (revolving) used. In *this* plan, though the number of reversals must equal the number of radii, yet the radii themselves being *compounded of a number of bars* (they need not of course be confined to the small number six as in the fig.), the power of the machine will (we may reasonably expect) be proportionally increased, whilst all *complexity in the commutator is avoided.*

The commutator I have adopted is that of Jacobi, figured in your first vol. with some modifications in its forms, whereby greater symmetry of appearance and equal certainty of effect is secured.

I am, Sir, yours,

June 12th, 1838. F. LOCKEY.

---

## V. *Researches in Electro-dynamics, experimental and theoretical. By* WILLIAM STURGEON, *Lecturer on Experimental Philosophy, at the Hon. East India Company's Military Seminary, Addiscombe, &c. &c.**

### *Description of another magnetic electrical machine.*

This machine is a modification of one first made by M. Pixii, and, with the exception of the discharging apparatus, fig. 12 plate II., is not very different to that exhibiting in the Adelaide rooms. A horizontal section of it is seen in fig. 13: where *m, m, m,* is the magnet already described as belonging to the former machine, fig. 1, plate I., vol 2: and *n, n,* is another smaller magnet, also of four bars, which fits inside of the larger one. Both magnets were made from bars of the same dimensions, and consequently, when fitted together, are of the same thickness.

The piece *b o a* is of soft iron, and rotates in front of the magnetic poles, on the spindle *s s*. The branches *b* and *a*, are cylindrical, and carry each a coil *c c*, of three hundred feet of copper wire covered with silk thread. The iron is made to approach the magnetic poles as closely as possible without touching, whilst revolving, past them, on its spindle.

By this means the iron undergoes a series of vicissitudes of magnetic polarity and energy during each entire revolution, and the magnetic force of the iron thus called into action becomes productive of electric currents in the coils.

The method of putting the spindle and appendages into motion, is by means of a wheel and band; and as the frame work is similar to that in fig. 1, plate I., vol. 2, it need not be again described.

* This paper is the latter part of a paper which was read before the Royal Society, on the 16th of June, 1836; the former part of that paper being already published in the second volume of these "Annals." See Vol. 2, page 1 to 24.

*Excitations of the coils.*

The plane of the soft iron, as represented in fig. 13, is coincident with that of the magnet; a position in which the magnetic force of the iron is at a maximum.

If the iron commence a revolution, in either direction, the magnetic force of both branches, *a* and *b*, will begin to decline, and the polarity to be less perfectly defined; and will continue to decline until the first quadrant of revolution be completed, or until the plane of the iron has become perpendicular to that of the magnet. In this position, although the magnetic forces be not neutral, they will be at a minimum, and the opposite sides of each branch of the iron will exhibit polarity of different kinds.

As the iron moves on, the extremities *a* and b, will change their polar character; which will progressively become more perfectly defined through the second quadrant of revolution. The magnetic forces will consequently improve until the branches *a* and *b*, be again opposite to the magnetic poles, and have changed places with regard to their first position; and similar vicissitudes of polarity and energy of force will occur during the passage of the iron through the other half circle of revolution.

If now, we permit the extremity *a*, of the soft iron, fig. 13, to move upwards, through a quarter of a circle, or from *a* 1, to *a* 3, in fig. 14, which is an end view of the circle of revolution, an electric current, whose direction is indicated by the arrows round *a* 2, *a* 3, would be excited in the coil. And, notwithstanding the change of polarity that would take place as the iron came into the position *a* 3, the direction of the current would continue the same until that extremity of the iron had performed half a revolution, and had arrived at the other pole of the magnet. For, in whatever direction the magnetic atmosphere of the iron travels through the coil in the first quadrant, it will proceed in the opposite direction in the second. So that if we conceive that it recedes in the former, it will advance amongst the convolutions in the latter; and as this magnetic atmosphere changes its polarity with respect to the iron, at the precise moment that it changes its direction of motion through the coil, the direction of the electric current will continue unaltered.

When, however, the branch *a* leaves the last approached pole of the magnet, and enters the third quadrant of revolution, there will be a recession of the magnetic atmosphere of the same kind of polarity as that which advanced through the second quadrant; and consequently the new current will be produced in the opposite direction.

This new current will continue during the time the iron is passing through the latter half circle of revolution, as indicated by the arrows round *b* 2, *b* 3, *b* 4, but will again change its direction as the iron passes the pole: so that in each entire revolution of the iron, there will be two opposite currents excited in the coil, which will regularly succeed each other as the iron passes the poles of the magnet.

The branch *b*, fig. 13, of the revolving iron will undergo similar vicissitudes of polarity in an inverted order; so that the currents in its coil will invariably proceed in the opposite direction to those in the other coil: but by a proper connexion of the extremities of the wires the currents may be made to conspire.

It will now be understood, that, as the electric currents change their direction through the coil at the precise time that the iron passes the poles of the magnet, those poles cannot be too well defined, in order that the transit of the iron past them, may be as sudden as possible. These views led to the construction of the compound magnet as represented in fig. 13.

The discharging apparatus fig. 12, is placed on the spindle fig. 13, in such manner, that the semi-wheels may succeed one another in the polar cells when the changes take place in the direction of the currents; or when the extremities of the iron are crossing the plane of the magnet.

EXPERIMENTS.

*Magnetic.* The following table will show at one view the magnetic deflections, with three different velocities of the coils. The galvanometer is that used in the first described experiments with the other machine.* The needle having the same directive force in both sets of experiments.

| Coil revolved to the | Deflections of the Magnetic Needle due to | | |
|---|---|---|---|
| | Three Revolutions per Second. | Six Revolutions per Second. | Twelve Revolutions per Second. |
| Right. | 55° | 55° | 57·5° |
| Left. | 50° | 55° | 57·5° |
| Mean. | 52·5° | 55° | 57 5° |

* See Vol. II. page 8.

With the greatest speed of the wheel, the needle could not be steadily deflected to 60°.

The deflections by this machine do not increase proportionally with those of the other, with the same speed of the coil. The reason appears to be this: that as the excitation of the coils depends upon the magnetic force developed by the revolving iron, the polarization of the latter is not so complete when the velocity is considerable as when it moves at a slower pace, and this diminution of magnetic force will necessarily abate the excitation whenever the iron moves with great velocity. This circumstance may possibly be a formidable impediment to the improvement of this kind of magnetic electrical machines beyond a certain point; but it cannot possibly be any check whatever to the other kind, where the magnetic force is already formed, and permanently situated, and has no dependance upon the revolving part of the machine.*

The electric powers exhibited by the first described machine are certainly much below those exhibited by that with the revolving iron. But it must be remembered that the coil wire of the former is only one third of the length of that of the latter. This difference in the length of excited wire, in addition to the difference of the magnetic force employed in the two machines, may possibly account for the difference of excited electricity. Should this supposition prove a fact, we shall be enabled to carry on the improvement to a very great extent and in a very simple manner; and large machines may be made of almost any required power.

*Chemical.* A solution of hydriodate of potassa and starch placed in a rectangular glass box, such as is used in lectures for exhibiting decompositions &c., was brought into the circuit: the terminal metals being slips of platina, with a gauze partition between them. The slightest motion of the wheel gave indications of liberated iodine at the issuing terminal. Four or five turns produced a copious liberation, and ten turns were sufficient to produce a dense cloud, when the terminal metal was agitated a little to shake off the iodine with which it was loaded. Not a particle of iodine appeared at the re-entering metal.

By reversing the motion of the wheel, iodine was liberated at the other metal.

A solution of sulphate of copper placed in the circuit with platinum wire terminals, became partly decomposed by one turn of the wheel. Five turns of the wheel liberated as much

* Vol. II. page 1.

copper as made the re-entering terminal look like copper wire for half an inch of its length. But none was liberated at the issuing terminal.

A mixture of archil and solution of sulphate of potash was placed in a glass tube bent into the shape of the letter V, having a platinum wire in each branch and properly connected with the polar cells of the machine. By a few turns of the wheel, the liquor in the branch containing the issuing terminal wire, became quite red. That in the other branch a deeper blue than the original liquid.

Strong fuming nitrous acid, diluted with twelve measures of distilled water was placed in the circuit, having one platinum and one copper terminal. When the latter was made the issuing terminal, the dissolution of the wire was more rapid than by the acid solution alone. When the copper was made the re-entering terminal, the chemical action on the copper was perfectly annihilated.

Twenty feet of thin copper wire made into a small coil was made the re-entering terminal in a portion of muriate of tin properly placed in the circuit. The other terminal was a thin platinum wire. The whole twenty feet of copper wire was partially tinned by the machine current, in about ten minutes.

A similar coil of copper wire was afterwards tried, with five feet only at a time placed in the muriate. The first five feet soon became perfectly tinned. A second five feet also became tinned in its turn, and so on for five feet at a time till the whole twenty feet were covered with tin.

In this way the twenty feet of wire were better tinned, and in a shorter time than by introducing the whole at once into the muriate. This result was to be expected, because of the current being too much attenuated to produce rapid decomposition when the whole length of the wire was exposed to the muriate.

Distilled water, slightly acidulated by a few drops of sulphuric acid, was placed in the circuit in an apparatus of the usual kind for decomposing water by voltaic electricity; the platinum terminals of which, being one inch long and a quarter of an inch broad, and three quarters of an inch asunder. A glass tube filled with the same kind of acidulated water, was inverted over each terminal. The glass tubes are of the same dimensions, each half an inch in diameter; consequently exposing a transverse sectional area of $\frac{11}{56}$ of an inch. Then $1 \div \frac{11}{56} = 5\frac{1}{11}$ inches in the length of either tube, will be the measure for one cubic inch of capacity.

The coils of the machine were put into motion at the rate of eighteen revolutions per second. At the expiration of

twelve minutes $3\frac{1}{2}$ inches of the tube over the re-entering terminal was filled with hydrogen; and $1\frac{3}{4}$ inches of the other tube was filled with oxygen. The sum of these is $5\frac{1}{4}$ inches. Hence something more than one cubic inch of gas was liberated in twelve minutes.

From the results of several experiments of this kind, I consider that, with these terminals, and water similarly acidulated, one cubic inch of gas is the average quantity liberated by this machine in every twelve minutes it is kept in motion, the coils making eighteen revolutions per second. With terminals of other dimensions, and with differently acidulated water, the results would be somewhat different.

Having thus ascertained what may be termed the standard power of the machine in decomposing water, comparative experiments were next made with a voltaic battery.

The battery which was employed is of the Cruikshank form, containing fifty-five three inch plates.* It was charged with rain water mixed with one thirtieth of nitro-sulphuric acid, half of which was strong fuming nitrous, the other good sulphuric. The decomposing apparatus being already supplied with acidulated water of precisely the same character as before, was placed in the circuit the moment the battery was charged; and the following results were noticed.

For the first five minutes the battery liberated gas much more copiously than the machine, but its powers began to decline very perceptibly at the end of that time. When it had been in action on the water for twelve minutes, the portion of the hydrogen tube filled with gas was measured, and was found to be $3\frac{1}{4}$ inches long.

At the end of the next twelve minutes, the hydrogen column was 4 inches long.

At the end of the third twelve minutes, the hydrogen column was $4\frac{1}{2}$ inches long.

At the end of the fourth twelve minutes, the hydrogen column was $4\frac{7}{10}$ inches long.

At the end of the fifth twelve minutes, the hydrogen column was not quite $4\frac{8}{10}$ inches long.

Now, as the hydrogen in one tube is in a constant ratio with the oxygen in the other, the differences in the lengths of the hydrogen columns will be an accurate measure of the decomposing power of the battery in each interval of twelve minutes. They are as follow.

* The breadth of the metal exposed to the exciting solution is about two and a half inches, the rest being covered with cement.

| | Decomposing power. |
|---|---|
| First interval | 3.25 |
| Second | 0.75 |
| Third | 0.5 |
| Fourth | 0.2 |
| Fifth | 0.1 |

At the end of the fifth interval, or in one hour from the commencement, the decomposing power was nearly extinguished.

By referring the constant power of the machine, or, which is the same thing, the constant quantity of gas which it will liberate in any interval of twelve minutes to unity, the quantity of gas, in cubic inches, liberated by the battery in the successive intervals will stand as below.

| | Quantity of gas in cubic inches. |
|---|---|
| By the machine in one interval | 1.000 |
| By the battery in the first interval | 0.956— |
| second | 0.220+ |
| third | 0.147+ |
| fourth | 0.059— |
| fifth | 0.029+ |

And by taking the whole hour as the interval of time, the power of the machine to that of the battery, in liberating gas, is as 5 to 1.411.

The metals of the battery were rolled copper and rolled zinc, and the pairs about the fifth of an inch apart. This latter circumstance tends to shorten the period of action; but exalts the electric powers at the commencement: and rolled zinc has an advantage over cast zinc. A stronger solution would also have exalted the electric powers of the battery for a short time; but then the metals would have been destroyed faster, and the action of shorter duration.

In whatever way we compare the experiments here detailed, it is obvious, that the results are favourable to the employment of the machine. Its powers are constantly the same, which is a material advantage both in experimental and theoretical investigation.

The shock from this machine is exceedingly disagreeable, and the sparks which are seen in the polar cells are brilliant, and accompanied by a snapping noise.

Should these facts meet with a favourable reception by the Royal Society, my object will be completely attained. The magnetic electrical machines, when their powers are once known, will soon find their way into the hands of experimental philosophers, and by future improvements may become a more formidable implement of analysis than any hitherto placed at their command. *Artillery Place, Woolwich,*

*June* 15, 1836.

APPENDIX.

During the time I was carrying on the experiments described in this paper, the mercury with its cells were occasionally removed from the machine, and spring *dischargers*, made of brass wire, pressing on the peripheries of the semi-wheels, and lubricated with sweet oil, were made to replace them. A few trials soon convinced me of the advantage likely to be gained by this mode of discharging the currents; but before I had got any series of experiments with this apparatus properly arranged, I was made to understand that my paper would not be permitted to appear in the Transactions of the Royal Society; a circumstance which appeared to me no very great inducement to proceed any further with the investigation, at least at that time.

Mr. Christie, one of the council of the Royal Society, saw some of these experiments, with the spring discharger attached to the machine, at my house, within a week after my paper was read. At that time the machine would liberate one cubic inch of gas, from acidulated water, in eight minutes; showing an increase of power equal to one third of that which it exerted with the mercurial polar cells.

During the latter part of 1836, I varied the shape and size of the revolving armature, still employing the same six hundred feet of copper wire for the coils. With this new armature the machine liberated one cubic inch of gas in five minutes;* consequently its decomposing power was more than double that which it first exerted with the mercurial cells. During the last winter I again varied the shape and size of the armature, and also the length and size of the coil wires, by means of which the decomposing power of the machine was again improved. Since that time I have attached hollow armatures, and also compound armatures, made of bundles of thin iron rods, and iron wire of various dimensions, all of which vary the power of the instrument.

I have for some time past been engaged in a series of experiments for ascertaining the comparative decomposing power of this machine in its present form, with the powers of the various voltaic batteries now in general use. A full description of the machine, with these experiments, will shortly be placed before the readers of these "Annals." In the present

* I have not yet seen in London a magnetic electrical machine possessing one-tenth the decomposing power here stated; although that at the Adelaide Gallery is more than five times heavier than mine. Those usually sold in instrument makers' shops do not exhibit one-hundredth part of its power as a decomposing instrument.

instance I have strictly adhered to the paper in its original form; and have neither altered nor added one sentence since it was returned to me from the Royal Society.

---

## VI. *Instructions for the scientific expedition in the North of Europe. Part relating to the Phenomena of Electricity. Drawn up by* M. Becquerel.*

Electrical phenomena have become of such importance, on account of their connexions with a great number of natural phenomena, that we must take them into consideration in studying the latter: travellers also, whatever may be the part of the globe they traverse, ought to try to discover if such and such phenomena, which pass under their observation, have or not an electrical origin, or at least if electricity intervenes in any manner in its production.

We are going to point out several series of experiments to be made, not only in the north of Europe, but in every other locality. We shall divide these series into three classes: the first will be relative to the electricity of the atmosphere; the second to electricity in motion and its use; and the third to electricity acting as a chemical force.

*Atmospheric electricity.* The atmosphere, in calm weather, when no disturbing cause mixes the different strata of air situated at a certain distance from the earth, is a vast reservoir of positive electricity, whose intensity, which is increasing from the surface of the earth to a certain height at present undetermined, is subject to variations which give two *maxima* and *minima* every twenty-four hours.

This excess of electricity, which is very weak before the rising of the sun, increases by degrees with its elevation, then rapidly, and usually some hours after, arrives at its first maximum. This excess diminishes at first rapidly, then slowly, and arrives at its minimum some hours before the setting of the sun. It begins to mount again as soon as the sun approaches the horizon, and attains its second maximum in a few hours after, then diminishes till the rising of the sun. It then follows the plan before mentioned.

We know from observations made with great care by Schubler, that the intensity of the electricity for the two maxima and minima increases from the month of July to that of January inclusive, so that the greatest intensity takes place in

* From the Compte Rendu Hebdomadair des Séances de l'Académie des Sciences. April 23, 1838. Translated by Mr. J.H. Lang.

winter, and the least in summer; we also find in the winter months that on calm days the increase of the electricity is always in proportion to that of the cold. It would be interesting to see if we should obtain similar results in the polar regions during the long winter nights, for which time the state of the atmosphere experiences little variation, so as to know how far the fall of the dew, the formation of the terrestrial vapours, and the electricity of the earth influence the phenomena observed.

When the weather is cloudy, the free electricity which is found in the atmosphere experiences great variations, both in its nature or in its intensity. During storms, when it rains or snows, the electricity is sometimes positive, and at others negative; and its intensity is much more considerable than in fine weather. We have not yet been able to establish any law on the nature of the electricity under such circumstances; but experiment has proved that, in the course of one year, there have been nearly as many negative as positive days. It would be desirable for somebody to attend to the following ex-experiments, during the days on which the atmosphere is troubled, to see if we could not find some connexion between the electricity of the atmosphere and the physical effects which are then manifested. These experiments may be made with different apparatus, the description of which will be found in the work of one of your commissioners. *Traité de l'Electricité et du Magnétisme.* T. IV. p. 79 *à* 85 *et* 107.

Several local causes generally make the intensity of the atmospheric electricity differ even when the sky is serene. This electricity is generally stronger in the most elevated and insulated places, not at all in houses, under trees, in streets, courts, and generally in places enclosed on all sides. It is, however, perceptible in towns, in the middle of large places, at the borders of quays, and principally on bridges, where it is stronger than in open fields.

This is what takes place in our climates; we ought to prove whether it be the same, as might be presumed, in others.

On the other hand we know that the atmosphere and earth are continually in two different electrical states. These two electricities must continually combine in the lower strata of the atmosphere, by the intervention of bodies which are found on the surface of the earth. In open fields, experience proves that, in any length of fine weather, we commence finding positive electricity of only one metre, or about $1^{m.}$ 3 above the ground. Hence the recomposition of the two electricities takes place at this height, when no foreign cause disturbs the state of the atmosphere. From thence the electricity expands

in the air, following a law we are not acquainted with, but which depends on the bad conductibility of its constituent parts and different causes of which we have no idea. It is difficult to find the algebraic expression for this law, varying every moment on account of the vapours which arise from the sun, or descend on the earth: but if it be wished to have approximate values of the electric intensity in proportion as it becomes higher in the atmosphere, the process used by M. Breschet and myself, at the Great St. Bernard, may be employed. We extended on the ground a piece of thin gummed silk 3 metres long and 2 wide, on which was unrolled a silk wire covered with tinsel 80 metres long. One of the ends of this wire was put in connexion with the stock of a straw electrometer, by means of a running knot slightly pressing the stock: the other end was fixed to the iron spear of an arrow: we then shot it from a strongly extended bow. The arrow as it ascended took with it the wire, which, being feebly attached to the stock, separated as soon as it was unrolled. The straws were separated by degrees in proportion as the arrow ascended: the separation soon became such that the straws struck powerfully the sides of the bell. The wire being separated from the stock, the apparatus preserved the electricity that had been communicated to it, which was positive. We do not doubt that by this we may be able to charge a condensor sufficiently to give sparks, even in the ordinary time. We perceive that, by this process and with good electrometers, we may approximately estimate the intensity of the atmospheric electricity at different heights above the ground.

To determine that the electricity submitted to the apparatus is not owing to its friction against the air, it is sufficient to draw the arrow horizontally, three feet above the ground, and to see if any effects are obtained; usually we have none.

Instead of an arrow a balloon may be used, furnished with convenient necessaries, which are attached by a conducting cord, whose lower extremity communicates with an electrometer. This mode of experimenting is not so simple as the preceding, as we have not always the means of obtaining hydrogen gas at our disposal; and the lateral currents of the air carrying the balloon, prevent its rising vertically.

We advise experimenters to be on their guard against an effect observed by MM. Gay Lussac and Biot in their aerostatic voyage, seeking to give an account of the distribution of the electricity in the elevated regions of the atmosphere, by means of a metal wire 50 metres long, terminated at the bottom by a metal ball, and attached by the other end to the car; they observed that while the weather was very serene the

electricity was negative. This result was in opposition to the fact already well proved, that the atmosphere always possesses an excess of positive electricity when the sky is without clouds. M. Biot has given an explanation of this fact, to which we would refer travellers. *Traité de l'Electricité et du Magnétisme.* T. IV. p. 112.

The electricity which belongs to the earth may be recognised by employing M. Peltier's process, a description of which may be found in the *Traité de l'Electricité et du Magnétisme,* (T. IV. p. 107). This electricity yields to some effects, which were first observed by Tralles, then confirmed by Volta, and one of your commissioners.

Tralles being one day on the Alps, opposite a cascade, presented his atmospheric electrometer, not armed with the metallic rod, to the very fine rain which results from the scattering of the water. He immediately obtained very distinct signs of negative electricity, even during calm weather, and when the free electricity of the atmosphere was positive. Similar effects have been observed in the neighbourhood of several cascades.

We are led to believe that the water, by falling with a great velocity on the rocks, is scattered into vesicular globules, which carry with them into the air the negative electricity that they have brought to these rocks and consequently to the earth. This electricity could not be attributed to evaporation, since it is of a contrary nature to that which this action produces. We recommend travellers to repeat these experiments near cascades, and to study the effects produced so as to be able to give a complete explanation of them.

As soon as the earth possesses an electricity of a contrary nature to that of the air, when the latter is calm, it follows that the clouds, which are always more or less electrized, experience different kinds of action on the part of the mountains. We particularly call the attention of travellers to the electric state of the parasite clouds which collect about the summits, which clouds seem to exercise over them an attraction to which the action of electricity could not well be foreign, as we might be led to believe from the following fact: M. Boussingault observed in the Andes parasite clouds of immense size, and which were attached to the upper part of a cone of trachyte. They adhered to it, and the wind could not detach them: the thunder burst this mass of vapours, and the hail mixed with rain immediately inundated the base of the mountain. Hence we may conclude that the great quantity of electricity which the clouds encircling the tops of these mountains

possess, exercise over them an attractive power, as long as the discharge was not effected. Researches on this subject would not be without interest for the natural philosophy of the globe.

During the appearance of the aurora borealis it would be well to determine, whether the electric state of the atmosphere, in calm weather, did not experience particular variations.

*Of fulminary tubes.* When thunder falls on any point of the surface of the earth, it always follows the best conducting bodies to attain water. The bodies are split, burnt if they are combustible, or broken, according to their nature and the intensity of the discharge; but if, in order to attain this water at a considerable distance below the ground, it be necessary to traverse larger or smaller masses of sand, it produces vitrified tubes, called fulminary tubes. This effect takes place particularly in sandy plains deprived of trees and houses. When an opportunity offers, it would be well to collect all the information relative to this phenomenon, and to follow as much as possible the direction of these tubes as far as the water itself, so as thoroughly to understand all the circumstances of their production.

*Use of electric currents in determining the temperature of the bodies of men, animals, and vegetables.* Hitherto, common thermometers have been used for the determination of the temperature of the interior parts of the bodies of men, and animals, but their utility is very limited; for if an incision be made for the introduction of the apparatus, a disorganization is produced, and, consequently a disturbance in the vital functions. To obviate this inconvenience we use needles each composed of two others, one copper and the other steel, soldered at one of their ends. Let us suppose these two needles to be placed in communication by their copper side, with the two extremities of the wire of a very sensible multiplier, and also by their steel end with a steel wire. When the temperature is the same in the two solders there will be no thermo-electric effect; but however slight a difference there may be, even 1-10th of a degree centigrade, it immediately shows a current in favour of that solder which has the highest temperature.

We will now suppose that one of the needles be introduced into a muscle by the process of acupuncturation, the solder being in the middle, and the solder of the second needle placed in a source of heat whose temperature is constant, the direction and intensity of the current will show the difference of temperature between the two solders, and, consequently, the temperature of the muscle. The effects being instantaneous,

this process is, therefore, exceedingly fit for discovering the changes of temperature which are exhibited in physiological phenomena.

The source of constant temperature is furnished by M. Sorel's apparatus, described in the *Traité de l'Electricité et du Magnétisme*, (T. IV. p. 13) or by the mouth of a person accustomed from previous attempts to keep one of the solders in the same position between the mouth and the palate. The precautions to be taken in estimating with exactness the temperature of the interior parts of men and animals, may be seen in the memoir lately communicated to the Academy, by M. Breschet and one of your Commissioners.

It has been stated that the temperature of these parts diminishes in going from the poles to the equator. We have requested the philosophers, who are going to the north of Europe, to vary their experiments, so as to determine this fact.

They may also estimate the value of the interior temperature of trees and bushes. The same apparatus will serve equally for determining the temperature of the earth and the variations it experiences, even to a depth limited by the probes for perforating the earth, that the expedition will have at its disposal: we should also advise them to use M. Melloni's galvanometer and reflectors, to discover if the aurora borealis emits towards the earth appreciable rays of heat, and to employ the thermo-electric apparatus whenever it will act, for appreciating the spontaneous changes of temperature, since there are no instruments as delicate for appreciating similar effects.

*Polar magnetism of the mountains and electro-chemical phenomena.* Since the application of electro-chemical effects to the explanation of several geological phenomena, a vast field is opened for persons wishing to study the relationship which exists between them. We are going to state some questions for them to solve, which are not unimportant for the natural philosophy of the globe.

M. Humboldt is the first who has proved the polar magnetism of a schistus and serpentine mountain in the Heidelberg.

The remarkable part of this magnetism is the distribution and parellelism of the axes. The homonymous poles occupy a similar declivity. M. Lichtemberg has started the conjecture that these axes might very well be the effect of earthquakes, which, in different parts of our planet, have for a long time acted in a similar direction.

In fact M. Humboldt has once seen the magnetic inclination in America changed in consequence of an earthquake. After this there can be no doubt that the magnetic axes of the mountains possessing polarity, equally experience changes

from the effect of earthquakes: hence, it would be desirable to ascertain if the direction of these axes be constant, or whether they change with the direction of the magnetic meridian of the country.

The axes of the magnetic mountains being determined, we must examine, whenever the rocks which constitute them are in decomposition, if the parts which have a similar polarity are in the same state of decomposition, taking a very extensive account of their exposure to the winds. In the case in which the parts not possessing the same polarity present differences, these should be noted, and also the products formed, so as to trace them to the causes which have exercised a determining influence over them. The same observations may be made with regard to the mountains of granite, gneiss, or other rocks which are in decomposition: that is to say, we should seek with care whether all parts of the mountains similarly placed, with regard to the magnetic meridian, are found to be in the same state of decomposition.

These observations extend equally to the changes which operate in the ancient galleries of mines.

There yet exists many observations to be made on the decomposition of the rocks in which the electric forces take a certain part, or at least the effects of contact, whose influence cannot be doubted. We would in this respect refer to the fifth volume of the *Traité de l'Electricité et du Magnétisme*, p. 185, and the following pages.

*On the existence of electric currents in veins of metal.* We are led to believe there are electric currents traversing the small metallic veins, conductors of electricity, which establish a communication between the unoxidized part of the globe and the liquids which come from the surface by interstices, and whence an energetic chemical reaction results, of which the volcanic eruptions are an evident proof. During this reaction the unoxidized part takes the positive, and the oxidized the negative electricity. These electricities re-combine by the intervention of all conducting bodies that are in their neighbourhood; these electric currents are ramified probably in all the small metallic veins. Hitherto we have not been able to demonstrate in a manner exempt from objections the existence of these currents, because we have not taken the necessary means of guarding against the causes of error. We have here a new series of researches of the highest importance, which we recommend to the philosophical travellers who are about to visit the works of mines. We refer for more ample information on this head to the *Traité de l'Electricité et du Magnétisme* (T. V. p. 165, and following, 201 &c.)

The metallic veins which are probably traversed by electric currents, are interrupted in a thousand places by rocks which are non-conductors of electricity, forming as many solutions of continuity necessary that the currents react chemically on the constituent parts of the liquids or solutions which moisten both the veins and the protuberant minerals. From whence result many decompositions and new combinations, whose nature depends on that of the principles which are present; we reason here very extensively in the hypothesis in which the outside of our globe would be furrowed in all directions by electro-chemical currents, whose existence although not yet recognised in an incontestable manner by experiments, are nevertheless admitted in theory.

The researches that we recommend to MM. the members of the scientific expedition in the north of Europe are rather delicate; they require an acquaintance with apparatus of very great sensibility, the use of which is not well known. If, therefore, they wish to avoid all the causes of error, we should advise them to multiply their experiments before commencing their work of research.

---

VII. *Description of three different instruments for opening and shutting the battery circuit of an electro-magnetic coil machine. By* William Sturgeon, *Lecturer on Experimental Philosophy, at the Hon. East India Company's Military Seminary, Addiscombe, &c. &c.*

*First instrument.* In the usual mode of opening and shutting the battery circuit by the wires of a revolving iron magnet between the poles of a permanent horse-shoe magnet, there can never be more than two interruptions during each revolution of the iron. These interruptions of the circuit occur at the partitions of the vessel which divide it into two cells for holding the mercury as poles to the battery. It is thus that the sparks are exhibited at two points of the circuit only, both of which are in the plane of the vertical horse-shoe magnet; and for the same reason only two shocks can be given from a coil machine, for each revolution of this kind of discharger. Hence it is that this instrument is not well calculated either for the exhibition of sparks, or for the discharge of a rapid series of shocks; for the latter purpose, however, the object might be accomplished by employing an active battery, which would produce a more rapid rotation of the electro-magnetized iron. Notwithstanding, however, these defects in the instrument, it is, without exception, the most

convenient mode of opening and shutting the battery circuit, either for medical or other purposes, where an assistant is not at hand, or whose company is not wanted, that has yet appeared before the public.

The instrument I am about to describe I hope will be found to possess some advantages over that just mentioned, and can hardly fail to become generally employed, both at the lecture table, and as an useful appendage to the electro-magnetic coil machine when used for medical purposes. Figs. 15 and 16, Plate II. respectively represent a horizontal and vertical plan of the instrument. The part *b, b, b,* represents a round wooden base, in the upper part of which is an annular groove for the reception of mercury. The surface of the wood in the centre of the groove is in the same plane as the outside rim, and about one inch diameter. The groove is about three quarters of an inch broad, and half an inch deep. It is separated into two compartments by two low wooden partitions *p, p,* which are glued to the bottom and sides of the groove, diametrically opposite to each other, and are about two-tenths of an inch high. Besides these partitions there are two *half*-partitions, one in each compartment. These half-partitions are glued to the bottom and inner edge of the groove, and project a little more than half way across it. Across the underside of this box, and parallel to the two whole partitions, is cut a groove for the reception of the central part of a staple shaped steel magnet *n, g, s,* whose extremities rise vertically on the opposite sides of the instrument, to about an inch and a half above the surface of the wood.

A piece of cylindrical rod iron, about 4-10ths of an inch diameter, and a little shorter than the distance between the magnetic poles, having a pivot in its centre, is covered with a coil of varnished copper wire, whose extremities hang down into the groove when the iron is properly placed on its pivot in the centre of the instrument. The iron, thus mounted on its pivot, is susceptible of rotatory motion in the plane of its own axis, between the poles of the permanent steel magnet *n, s*; and whilst revolving, the extremities of the coil wire just skim the partitions and half-partitions without touching any of them.

When this instrument is used, mercury is to be placed in its two compartments, until its surface will admit the immersion of the depending points of the wire which covers the iron, without its covering the partitions and half partitions over which the points have to travel. The ends of the wire which dip in the mercury are to be amalgamated previously to the mounting of the iron. The poles of the battery are to be

connected, by wire, with the two portions of mercury in these indented compartments, one with each, observing that the coil to be employed be properly placed in the circuit.

With this instrument the mounted piece of iron will rotate upon the same principles as that in the one hitherto in use; but instead of having only two places in the circuit for the exhibition of sparks, we have here four places, because of the circuit being opened at both the *whole* partitions and at the *half* partitions: moreover, when the *half*-partitions are properly disposed in the compartments, each of them will cause *two* interruptions of the battery circuit in one revolution of the iron, and consequently *four* interruptions are obtained from *both* half-partitions. Hence, the whole number of interruptions in one revolution is six; and as a spark or a shock is obtained at each interruption, we have six sparks, or six shocks, or both, produced for each revolution of the iron. By employing two more half-partitions properly disposed in the compartments, one in each, we obtain four more sparks or shocks in one revolution of the iron. By employing two more half-partition in each compartment we obtain ten sparks and ten shocks in the same time that two would be obtained by the old instrument. Moreover, there being twice the number of sparks at the half-partitions of those exhibited at the whole partitions, the light is proportionably brighter; and as they are not in the least concealed by the poles of the magnet, they are seen to much greater advantage than those in the plane of the magnet. With *one* jar battery and a good coil, the sparks exhibit a very beautiful spectacle. With four or six jars in series, the group of electric stars are really grand.

I have never yet used more than six half-partitions, but it is obvious that they may be multiplied much farther; each pair giving four sparks or shocks for every whole revolution of the iron. In point of exhibition, the apparatus is much improved by having a glass vessel for the mercurial compartments; and by having three or four half-partitions in each compartment, the dish is filled with brilliant electric light, emanating from a circle of refulgent stars, which dance on the mercurial margins in the most fascinating and imposing manner; whilst the singular noise which attends their exhibition indicates the rapid transmission of the subtle element through the intervening resisting medium. Another form which I have given the apparatus, shows the sparks at only *one* of the points of the revolving wire.

*Second Instrument.* In the first volume of these "Annals,"* I described an apparatus consisting of a notched zinc disc and

* See Vol. I. page 480, and fig. D, plate XV.

a revolving spring which passed over the notched surface, exhibiting a circle of sparks whose number in each revolution of the spring was equal to the number of notches in the disc. The zinc disc was soldered to a notched copper disc, a little larger than itself; and the copper disc to a notched one of iron still larger than the copper, forming altogether a kind of metallic rosette; so that by having springs of different lengths, corresponding to the different distances of the metals from the centre, the colour of the light exhibited on the respective metals was easily shown. Since that time I have modified this apparatus in two different ways, which I will now describe.

The first modification of this instrument consisted, chiefly, in substituting rings of the various metals for the discs in the original instrument. These rings consist of iron, copper, brass, zinc, tin, lead, bismuth, and antimony, which fit one into another, and are soldered together in the order they are named, constituting one compound annular disc, which is notched in eight radii from the outer to the inner metal, as represented by fig. 17, plate II. When this instrument is employed, the different spring pieces are of the same kind of metal as that against which they rub on the face of the disc. They are fixed at one end in a flat hole in the revolving axle by means of a screw with a milled head, the outer end pressing against the face of the disc. With this apparatus the spring pieces have to be changed with every variation of the experiment, in precisely the same manner as with the original instrument; but the rings forming the compound disc admit of a greater variety of metals than by having a complete disc of each kind, and the instrument is much more elegant. Finding also that the sparks are much finer when the spring moves over the disc at a moderate than a great speed, I have done away with the multiplying wheel and bands, which are always troublesome to keep in order; so that now the revolving springs are attached to the same axle as the handle or winch is attached to.

*Third Instrument.* This form of the apparatus affords a better opportunity of comparing the different coloured light exhibited by the deflagration of the various metals on the face of the disc than any other form I have yet given to it; because of the deflagration of the whole of them going on at one and the same time. The face of the disc over which the springs pass is represented by fig. 18, plate II. The disc itself is of brass; its face being studded with those metals whose electro-deflagrations are intended to be exhibited. The metallic studs are arranged on the face of the disc in two semi-spirals, as seen in the figure. These studs are soldered to the

face of the disc, and rise about a quarter of an inch above its plane, or stand in relief about that much. These studs are of the following metals, and stand upon both sides of the face of the disc, from the margin towards the centre, in the order they are named. They are iron, *i*, copper, *c*, brass, *b*, zinc, *z*, silver, *s*, antimony, *a*, tin, *t*, bismuth, *b*. The initial letters indicate the position of the respective metals. The disc is fixed vertically to a pillar attached to the coil apparatus; and a spindle, *a*, furnished with a winch behind, passes through the central opening to some distance in front. To that part of the spindle projecting in front of the disc, and at right angles to it, is attached a metal arm, which carries eight steel springs. These springs are fixed to the arm at distances from the centre of motion, corresponding to those of the metallic studs, which they are intended to touch. The outermost spring is steel throughout; and reaches to the iron or steel studs *i i*. The next spring is tipped with copper, and reaches to the copper studs *c, c*. The next spring is tipped with brass and reaches to the brass studs *b, b*. The next spring is tipped with silver and reaches to the silver studs *s, s*: and so on for all the rest. The springs are fixed to the arms in one plane which passes through the plane of its axis.

With this arrangement it is obvious that if the springs be rotated in the direction of the arrows, and commence from a vertical plane, one of the iron or steel studs will be the first at which the circuit will be opened. Hence a brilliant fiery scintillation will be seen at the edge of that stud; but the moment that the steel spring has quitted its fellow stud, the copper spring comes in contact with the copper stud; and when the copper spring has quitted its stud, the brass spring comes in contact with the brass stud: and in this manner the springs shut and open the circuit with their respective studs, in regular succession, from the outermost to the innermost metal, through one half of the circuit. When one half of the revolution is completed, the other series of studs are brought into play, closing and opening the battery circuit in the same regular succession as the former series.

When the springs revolve over the studs in the opposite direction, the series of metallic contacts are in the reverse order.

No language can convey a proper idea of the beauty of an experiment with this apparatus, when attached to a good coil and battery. It must be seen to be understood.

## VIII. *Homogeneous Attraction of Electricity. By* Charles Griffin, Esq.*

I beg leave to premise that, where, in the following observations I may appear to assert anything positively, which is but an inference, I mean only to say that so it appears to me at present, unenlightened as I am by the assistance and uncorrected by the criticism of others. I am anxious not to appear in any degree dogmatical, but quite open to receive, with pleasure, the friendly correction of my views from others who are more experienced in science. I feel deeply impressed with the truth of Voltaire's observation that "Quand on a fait une experience, le meillieur parti est de douter long tems de ce qu'on a vu, et de ce qu'on a fait."

There are several phenomena in electricity, which induce me to suppose that in its ordinary state it is more analogous to liquids than gases; and what I am about to advance in favour of its homogeneous attraction I think tends to establish that view of its nature, and consequently to remove the common idea of electrical atmospheres.

Many persons, in speaking of electricity as a fluid, I think regard it as a liquid, almost without knowing that they do so.

Several of the every-day appearances in electrical experiments appear to offer very strong evidence in favour of the homogeneous attraction and liquidity of electricity. Thus the ordinary spark in the discharge of a battery passes in one full round stream, and does not appear to swell out in the middle as we might expect it would if there were repulsion amongst its own particles. Such a stream will pass many inches through a vacuum without showing any sign of repulsion, but quite the reverse.

Again, the beautiful streams that pass across the electrifying machine exhibit just such appearances as we might expect from a fluid possessing the power of homogeneous attraction. These streams attract to themselves the particles of electricity from a considerable distance on each side their whole path, and thereby evidently increase considerably in size all the way to the rubber. This of itself seems almost a sufficient proof of homogeneous attraction.

The sparks at each link of a chain through which a battery is discharged, bear also strong testimony to the same effect. Each of these sparks when examined by itself appears to consist of a number of spirts or splashes of the electrical liquid,

* Read before the London Electrical Society on Saturday, June 2, 1838.

each spirt having a small ball at its extremity, and appearing much like a Snail's horn, as though the spirt when its projectile force was exhausted began to return into itself and form a ball at its extremity, just as we might expect from a fluid possessing the power of homogeneous attraction.

In this, and all other experiments, where there is a loud report, and a momentary glimpse only can be obtained of the effects, it is advisable to close the ears during the moment of discharge.

The diffusion of electricity in air and in vacuo forms no objection to its possessing homogeneous attraction; for such liquids as water, and mercury, and even solid metals, take the form of vapour at low temperatures, notwithstanding their powerful homogeneous attraction, and they exactly agree in this with electricity, for they take the vapourous form more readily in vacuo than in pleno. The spirts or splashes of electricity passing along a chain sometimes appear to take this form of vapour or spray. This spray and even the spirts may, and the latter, I believe, have been by some attributed to particles of metallic oxide, red hot, flying off from the chain.

In a beautiful experiment of Beccaria, the vaporization of electricity is readily exhibited on a ball of two inches diameter in vacuo.

I long since devised the following experiment on homogeneous attraction, but was unable to try it for want of an air pump, till February last. The sectional sketch of my apparatus, Plate IV. fig. 26, may serve to illustrate. *a a*, *b b*, are metallic caps cemented on *c c*, a glass tube about nine inches long, and *d d*, are short metallic tubes soldered into the cap *a a* to receive *e e*, *f f*, two glass tubes projecting each way about two inches from the metallic tubes. *p* 1, *p* 2, are two wires inclosed in the glass tubes, even with their inner ends but projecting beyond their outer ends. The inner ends are about one inch and a quarter apart. *h*, is a metallic tube and ball between six and seven inches from the inner ends of the wires *p* 1, and *p* 2.

The wires are cemented into the glass tubes *e e*, *f f*, and those into the metallic tubes. The cap *b b*, is furnished with a contrivance for applying the apparatus to the air pump to exhaust it.

I use two batteries of about two square feet each, made of common wine bottles, their exterior coatings insulated sufficiently from each other to prevent a spark passing from one to the other, and one of these is connected with the wire *p* 1, and the other with *p* 2.

The inner coatings of the batteries are connected with the rubber insulated. Both being charged negatively inside, are discharged at once by connecting one end of the discharging rod with the cap *b b*, and bringing the other into contact with the conductor which connects together the interior coatings of the two batteries.

By this means the two currents pass separately and simultaneously from *e* and *f*, but coalesce before they reach *h*, although their momentum in passing into the vacuum might be expected to resist such coalition. If there were neither momentum nor homogeneous attraction, the two currents ought to move in straight lines directly towards the centre of the ball *h*. If there were repulsion, they ought to form curves with their concave sides towards each other and enter the ball *h* on opposite sides.

My glass tube *c c*, is about two inches diameter, but it should be four or five inches to prevent the glass attracting the currents which it sometimes does.*

I have since made about 70 or 80 experiments in air, by passing the discharges of electricity over the surface of cards capable of receiving a permanent mark thereby. In the first thirty-seven of these experiments, I used the batteries before-mentioned, arranged in the same manner, and batteries of four feet surface each, in the others. A piece of small copper bell wire, about six or seven inches long, proceeded from each exterior coating, so that I could apply the loose end to any part of a card in order to cause a hole in it, or marks upon it by a discharge.

For the negative pole, I used sometimes a piece of wire similar to the before-mentioned, and sometimes a metallic plate. The cards I used were the common white and yellow enamelled visiting cards. The marks on the white cards are generally black in the middle when first made, but afterwards frequently fade. The following experiments may suffice to illustrate. *p* and *n* on the figures will usually signify the positive and negative poles. When near an arrow they will signify which current passed there. 1, 2, or 3, added to *p* or *n*, will signify the first, second, or third positive or negative pole or current. An arrow pointing towards a hole will show that

* I may be excused noticing, en passant, that this attraction of the glass suggests a probable mode of determining the relative attraction of different substances for electricity. Would not equal plates or balls of different substances, placed by the aid of micrometer screws at the precise distance, enabling them to attract to themselves an electrical current of a constant quantity and intensity, furnish a true measure of this power of attraction?

the current went through it. If from a hole, that the current came through it.

*Experiment* 1, made 18th March, 1838. Plate IV. fig. 27 *a b c*, is a notch cut out of a card with the intention of preventing the current from one positive pole being drawn by the metal of the other out of its course, supposing that if the currents coalesced at some distance from the poles, it could only be due to the mutual attraction of the currents. It appears by the track left by the fluid that the two currents coalesced by the current *p* 1, passing through the air across the notch to the current *p* 2, which it seems to have forced out of its direct path, and caused it to divide into three currents at *f*, two of them rejoining almost immediately at *g*. The third appears to have been attracted back again by the united first and second, and by its momentum, to have crossed that part at *i*, and finally to have coalesced with it and passed to the *n* pole.

*Experiment* 4, fig. 28, *a b c d* represent a notch in the card larger than in experiment 1, and for more effectually answering the same purpose. 2 *p* appears to have made a mark at starting, to have afterwards passed just under the notch to the 1 *p* current, which was first attracted and, after joining, thrown out of its course by the momentum of 2 *p*. In this, and experiment 1, a small spirt appears at *e e e*, as it generally does where the fluid suddenly changes its course, as might be expected from the fluid possessing momentum.

*Experiment* 14. 20th March, fig. 29. Notch as before. A very curious experiment. 2 *p* makes a dot as is often the case at the pole, then darts through the air across the notch to its left hand corner, in doing so it appears to have attracted a small portion of the 1 *p* current. This portion and the 2 *p* current appear then to cross each other by their momentum, and by their concussion to make a broad blotched mark fainter than those made by the direct currents.

*Experiment* 15, fig. 30. The lines show the notch, and broader than in the three former experiments. 2 *p* appears to have passed through the air across the corner of the notch a long way out of its direct course to come at 1 *p*, which makes a curve to meet it.

*Experiment* 18. Fig. 31. The line *a* represents a slit in the card: the two corners formed by it were turned up perpendicular to the general surface of the card, forming creases where the two other black lines are. The dotted line shows the deviation of the 2 *p* current from a right line. That current appears to have forced holes at *e* and *i*, and passed through both the raised corners of the card to get at the other current,

or that both currents passed through one hole, each to meet together between the turned up corners.

*Experiment* 19, fig. 32. The part, *a a*, of the card about one-tenth of an inch high across the whole breadth of the notch was raised perpendicularly. This appears to have driven the 2 *p* current under the card, where it divided into three currents; one, the larger current, coming through a hole which it made at *e*; a fainter current coming through a smaller hole at *d*; and a still fainter current through another hole at *c*. The three small dotted branching lines show the course of the divided 2 *p* current under the card. These three branch currents appear to have been attracted by 1 *p*, out of their course, in inverse ratio to their quantity, which agrees with the laws of other matter.

*Experiment* 20. Fig.33. Large notch and small piece of raised card, *a a*, as in the last experiment. The two currents appear to have met in the air where the piece of card was cut out, then to have struck the top of the raised bit of card and passed down it and so to *n*.

*Experiment* 21. Similar notch to the last, the 2 *p* current nearly straight but one part of the current left the other to meet the 1 *p* current, which it joined and then returned into the other part of the 2 *p* current, nearer to and about half an inch from the negative.

*Experiment* 22, fig. 34. In this and most of the other experiments, after the first four or five, the cards were fixed vertically by placing their lower edges in a notch at the top of a small wooden pillar, or between pieces of glass placed in the notch. *a b c d* show a part of the card included between two pieces of glass in order to bring the attracting currents as nearly as possible together, and to remove any doubt as to the matter of one positive pole interfering with the result by attracting the current of the other. 1 *p* was placed on the glass on the side shown in the figure, and 2 *p* opposite to it, so that the two currents had to pass the first part of their course, divided from each other by two pieces of glass and a card till they reached *e*, where 2 *p* made a hole, came through and joined 1 *p*, and with it passed to *f*, where they made another hole to get to the negative pole, which was on the 2 *p* side of the card; here the probability was much against 2 *p* passing through the card, *n*, being on the same side with its pole.

*Experiment* 23 and 24. Similar to the last in all respects, except that the 2 *p* in passing through to join 1 *p*, made three holes in experiment 23, and two holes in experiment 24.

*Experiment* 25. The negative on the same side as 2 *p*. The glass on the 1 *p* side came much nearer the negative than

the $2p$ glass. The $2p$ current passed straight to the $n$. At the edge of the $1p$ glass, the $1p$ current came through into the track of the $2p$, about the middle of its length.

*Experiment* 26. The negative pole on the $1p$ side. The edge of the $2p$ glass nearest to the negative, the $1p$ made no track till the $2p$ came through two holes to join it, they then jointly made a track till they came opposite to and passed through one hole to the negative.

*Experiment* 27. Two cards included between two pieces of glass as in experiment 22, and $1p$ and $2p$ placed as in that experiment. The $n$ pole between the cards within half an inch of the edges of the glass. $2p$ passed through both cards to $1p$, both together made a track and passed through one hole in the $1p$ card to the negative.

*Experiment* 28. 21st March. Six cards included between the pieces of glass, the negative pole between the two middle cards.

The $1p$ pole a little to the right of the point opposite to the $2p$ pole. The $1p$ current passed through one card, then turned very short to the left towards the $2p$ current, then through three other cards (thus passing the negative pole), it there met the $2p$ current. The $2p$ current passed over part of its first card without a mark, then divided its current; then the two parts coalesced and passed through its first card with one hole, through its second card making two holes, where it joined the $1p$ current, and with it passing between the second and third cards to the point opposite the negative where both passed through the third $2p$ card to it. Between the second and third $2p$ cards where the currents joined they made a large black mark half an inch long and a quarter wide.

Where these large marks or blotches are made it appears the current makes a strong black narrow track, and that it is the vapours and gases of the card which make the broader or fainter portion of the marks on each side the darker track.

*Experiment* 31. In this experiment half the $1p$ current appears to have left the other half to join the $2p$ current, and with it to have rejoined the other portion before it passed to the negative.

*Experiments* 32, 33, 34, 35, 36, 37, were with the poles reversed, so as to have one positive and two negative poles. In 33 and 35 the current of electricity appears to have divided immediately it left the $p$ pole. In 37 at about one-third of an inch from that pole. In 32 and 36 about half way, and in 34 above half way from the $p$ to the $n$ poles.

The separation of the current in these six experiments produced a shorter and sharper snap than the ordinary report,

and appeared more like the sudden rending of a strip of Indian rubber.

*Experiment* 40, fig. 35. The dotted line shows the course of the *n* pole touching the card on the other side.

The 2 *p* current first makes a dot, then starts as though it would go to the bend of the wire opposite to it. It then makes a large curve to get at the 1 *p* current which makes a sudden bend to meet it. At *a* is the very common appearance of a spirt accompanying a sudden change of direction, and at *b*, a light sputtering mark apparently from the same cause.

*Experiment* 44, fig. 36. In this the double lines *b b* represent furrows and the single lines *a a* ridges in the card, and a third positive pole 2*p* is added proceeding from a single powerful jar of nearly two square feet of surface. The 1 *p* current in its way towards *n* is suddenly arrested and turns over the ridge to the 2*p* current. The 2*p* current appears to have gone to join the 3 *p* current but was drawn out of that course a little way by the 1 *p* current. The 3 *p* current probably leaped from the ridge on its own side through the air across the furrow to *n*. It also appears probable that the coalesced 1 and 2 *p* currents joined it in the air and by their momentum forced it out of its straight line to *n* as would appear by the direction (shown by the dotted line) of the short track it left on the ridge there.

*Experiment* 43 was with the same batteries but with a flat card. 3 *p* made a track nearly straight to a point in the 2 *p* current near the pole. The 1 *p* and 2 *p* currents proceeded for about half an inch each towards *n* but leaning out of the straight lines joining their poles with the *n* pole so as to approach each other. Then the 2 *p* current joined by the 3 *p* current appears to have darted into the 1 *p* current and by its momentum forced that current far across the line joining the 1 *p* pole to the *n* pole.

*Experiments* 45, 46, 47, fig. 37. 22d March. One figure comprises these three experiments. The dotted lines represent ridges made by bending the card. In experiment 45 the poles were at the points of the arrows *a a a*, and the marks *s s*, *&c.* were made. In this experiment it might be thought the current *a* 1 *p* was attracted by the metal of the pole *a* 2 *p*.

For experiment 46, the poles were then placed at the points of the arrows *b b b*, but the battery 2 was disconnected from the joint conductor, and the battery 1 only charged and discharged. The current *b* 1 *p* made the mark *x x x* by which it appears it did not in this experiment deviate near so far from a straight line to the negative as *a* 1 *p* did in experiment 45, and as it will be seen it did in experiment 47.

For experiment 47, poles in the same places as in experiment 46, both batteries discharged as in experiment 45, and the tracks A A A made.

The attraction of the currents seems pretty evident from this experiment. *b* 1 *p* is first attracted towards the pole *b n*, then (probably on the appearance of the first portion of the current *b* 2 *p*), is drawn aside towards the pole *b* 2 *p*, then on the current *b* 2 *p* having stretched towards *b n* is attracted towards a point of that current nearer to *b n*, then divides into three by the opposite attractions of the one portion of the current *b* 2 *p* nearest to the pole *b* 2 *p*, and the other portion nearest to *b n*. The current *b* 2 *p* appears to have been drawn out of its course to meet the three branches of the current *b* 1 *p*.

*Experiments* 48, 49, 50, fig. 38. The poles in each experiment on the same places on the same card. The three dotted lines were slight ridges as in former experiments. In 48 and 50, the 2 *p* battery was disconnected from the negative prime conductor, and the 1 *p* only charged and discharged, making the tracks A A A. In 49, both batteries charged and discharged making the coalescing tracks *x x x*.

*Experiment* 51, fig. 39. This seems a very important experiment. 1 *p* was under the card at the point *f*, the dots at *f g h a b c* indicate the marks made by the currents under the cards. The double curve line at *n* represents the wire negative pole formed into a single bow. The 2 *p* current is drawn suddenly out of its course leaving the usual spirt at *d*. The 1 *p* current increasing in length, its centre of attraction is removed nearer to *n* and causes a bend in the 2 *p* current at *e*. The larger portion of the 2 *p* current then passes through the card, making a hole at *a*. A smaller portion is prevented by momentum from doing the same and passes on and divides: the upper portion, as if attracted by the larger portion already passed through, passes also through at *b* into, and *rejoins* it. The lower portion by its momentum and the attraction of *n* is curved downwards a little way, but is gradually turned back towards the larger portion of the 2 *p* current, and the 1 *p* current, all under the card, and passes through and joins them at *c*. The whole of both currents now pass on under the card, making the short mark, indicated by dots, at *h*, and come through to the negative by making the hole at the wire.

Here the homogeneous attraction by a fortunate accident was exercised by different portions of the *same* current upon each other.

*Experiments* 52 and 54. The wires were placed as in 51. In each case the current starting on the same side as the

negative curved pole passed through the card about half its way to that pole to join the other current, and then both passed through at the negative pole.

*Experiment* 53. The same arrangement. 2 *p* reached the curved pole *n* where it touched the *edge* of the card on the 1 *p* side. The 1 *p* current, in making a bend to get at the 2 *p* current, divided; but the two portions rejoined and came down to the *n* pole without reaching the 2 *p* current.

*Experiment* 55. Same arrangement. 1 *p* came through about half way down to join 2 *p*, and then passed to the *n* which was on that side.

*Experiment* 56, fig. 40. *a b c d* show the boundary of a piece of glass between two cards; 1 *p* was placed on the card as shown; 2 *p* on the other side the card and glass opposite the point so marked. The negative was a small curved wire between the edges of the cards as marked at *n*. 1 *p* appears to have made several attempts to reach the 2 *p* current which was under the cards and glass, and made and passed through holes in its own card at the points of the arrows *e* and *f* before it reached the edge of the glass. It then marked the other side of the card beneath the dotted line to the edge of the glass at *g*, where it passes through to meet 2 *p*; 2 *p* made no mark on its own card till it was joined by 1 *p*, when the joint current made a mark from the edge of the glass till opposite *n* where it passed through the 2 *p* card to the negative between the cards.

*Experiment* 57, fig. 41. *a b k l*, the edges of a piece of glass between two cards as in experiment 56; 1 *p* as it appears; 2 *p* beneath the unseen card at *h*. The *n* between the cards at 1. The line *h i* shows the course of the 2 *p* current outside the unseen card. 1 *p* current divides, and the upper portion makes two bends to reach 2 *p*, and passes through the 1 *p* card at *g*. Part of the lower portion of 1 *p* passed through at *e*, the other part does the same at *f*, and rejoins the part which passed through at *e*. The upper and lower portions then make separate tracks under the seen or 1 *p* card, and pass through the unseen or 2 *p* card a little way under the glass by separate holes opposite the respective intersections of the two small crosses; they there join the 2 *p* current and pass along the outside of the unseen or 2 *p* card, and through it opposite *i* to the negative pole.

*Experiment* 59, was made with four cards placed so as to form one square: over them were sprinkled brass filings. All the poles on the upper side 1 *p*, about four inches from 2 *p* and both from *n* between five and six inches. The two currents coalesced about an inch from *n*.

*Experiment* 60. Similar arrangement, except that 1 *p* and 2 *p* were about three inches apart, and each four inches from *n*. The currents coalesced about half way.

*Experiment* 65, fig. 42. The wire *p* poles were bent about half an inch from their ends, so as to point from each other; the 1 *p* pole placed as marked; and the 2 *p* pole beneath the point so marked. The *n* pole was the edge of a round plate of tinned iron, *n n n*, lying on the card. The dark mark *a b*, the spot between the arrow point and that dark mark, and the dark mark *c*, were all made under the card. The 1 *p* current made a very faint mark, and passed partly through the hole shown near the arrow point, and partly round the edge of the card to join the current 2 *p*.

*Experiment* 66, fig. 43. *n n n* the edge of the circular plate used in experiment 65. 2 *p* beneath the card where it made a track, indicated by the dotted line, and came partly through the three holes at *a* and the one at *b*, where it only left a small trace, as shown, and that after coming through. The shivered appearance of the joint current says much for momentum, concussion, homogeneous attraction, and liquidity.

If the above observations and experiments are not considered sufficient to establish the existence of the power of homogeneous attraction in electricity, I have other experiments devised, varying the above methods. Probably very long and large marks might be obtained by discharges over or between prepared cards or glass in vacuo.*

*Leamington Spa, Warwickshire,*
*May* 12, 1838.

---

IX. *Report as to the safety and efficiency of the invention of Joyce's Patent Heating Apparatus. By* J. T. COOPER, Esq. *Consulting Chemist to the Polytechnic Institution, Lecturer on Chemistry and Medical Jurisprudence, &c. and* WILLIAM THOMAS BRANDE, Esq. F.R.S. *Professor of Chemistry in the Royal Institution.*

In compliance with your letter addressed to me on the 10th of March last, I have undertaken an investigation of Joyce's Heating Apparatus, in relation to its heating powers, the quantity of fuel consumed in a given time to produce in an appropriate room a certain increase of heat, also the amount of contamination the air of the room sustains in a certain time,

* Mr. Griffin's paper was accompanied by a great many of the identical cards on which the experiments were made.—EDIT.

as likewise the deterioration of the air by the combustion of oil, tallow, spermaceti, stearine, and gas, with the view of estimating the comparative injurious effects of Joyce's stoves, and of other methods, by which heat as well as light are produced; and also of the amount of contamination the air undergoes, in places where a number of individuals are congregated, and in which no injurious effects are found to occur.

In the outset I may state, that the room in which the experiments have been conducted, is nearly 14 feet long, 13 feet wide, and 12 feet high, and, consequently, contains about 2000 cubic feet. It has a chimney, and a peculiarly accurately fitted and well constructed register stove, which, when shut, effectually closes its lower aperture. Whenever a particular trial was to be made, bags of sand were placed on the juctions of the window sashes, and also at the bottom of the doors, and every precaution taken to make it as air tight as could be.

I find that one of Joyce's stoves, the internal cylinder of which is six inches in diameter and fifteen inches high, with an inverted cone, having twelve holes, each a quarter of an inch in diameter, burns three ounces of the prepared fuel per hour, when the regulating apertures at the top are quite open; in one instance, with a particular kind of fuel (such as is not commonly sold) it burnt three ounces and 4-10ths.; but taking the average of a great number of trials carried on for days, its rate of burning is a fraction less than three ounces per hour; but in all cases the combustion proceeds without producing any of the unpleasant odour that occurs when charcoal of the ordinary kind is burnt in a similar manner.

In one instance, the stove was kindled, and at eleven o'clock in the evening was placed in the above-named room, the temperature of which was 62 degrees Fahr.; the room was then closed, and not entered till ten o'clock the following morning; I then remained in the room about an hour, the doors and windows being kept closed, and found that exactly thirty-six ounces avoirdupoise of the fuel had been consumed; and on testing the air taken from the upper, lower, and middle parts of the room, the greatest quantity of carbonic acid contained was three quarters per cent. The temperature had increased to 72½ degrees Fahr.

In another experiment, the stove was allowed to burn fifteen hours in the closed apartment, and at the end of that time, it had consumed forty-four ounces and a half of fuel; and the air of the room on being tested for carbonic acid, as before, was found to contain less than one per cent, and the temperature had increased 13 degrees.

These experiments have been made repeatedly, and always with the same results, excepting some slight differences in the increase of heat.

It can be demonstrated as follows, that each ounce of pure charcoal, when burnt, will produce a little less than two cubic feet of carbonic acid: for, one hundred cubical inches of carbonic acid is estimated to weigh 47 grains, and every 22 grains of carbonic acid is known to contain 6 grains of carbon; then as 22 is to 6, so is 47 to 12·82, which is the weight of the carbon contained in 100 cubical inches of carbonic acid; then if 100 cubical inches of carbonic acid contain 12·82 of carbon, 1728 cubical inches, or one cubic foot, will contain 221·53 grains of carbon: again, if 221·53 grains of carbon be contained in one cubic foot of carbonic acid, one ounce avoirdupoise or 437·5 grains will be contained in 1·97 cubic feet, which is so nearly two cubic feet, that, for my present purpose, it may be said that one ounce of pure charcoal will produce two cubic feet of carbonic acid.

If no change in the air of the apartment had occurred in the two cases before related, there should have been present in the first instance 72, and in the latter 89 cubic feet of carbonic acid, which would have made the per centage 3·6, and 4·45; whereas, in both cases, it was less than one per cent, thereby showing, that whatever care may be bestowed to render a room air tight, that it is not possible to accomplish it so completely as to prevent the escape of the warm air, through minute pores and crevices from the upper parts of the room, and the entrance of the cooler air at the bottom, for in no other way am I able to account for the difference observed in the quantity of carbonic acid produced, and that detected in the air of the room.

An imperial pint of good sperm oil will burn, in a well trimmed Argand's lamp of the ordinary size, about twelve hours; but I find by my analysis, that a pint of such oil contains 6333 grains of carbon, or nearly 14·5 ounces avoirdupois, making the quantity of carbon consumed in one hour, a trifle more than 1·2 ounce; which, as I have shown above, is equivalent to the production of 2·4 cubic feet of carbonic acid. It will follow from this, that two such table lamps burning together will produce nearly as much carbonic acid in the same time, as one of Joyce's stoves, such as I have used in my experiments, and which I have before stated to be adapted for warming an apartment, containing about 2000 cubic feet of air.

A moulded tallow candle (long four) burns, on the average of some hours, 122 grains of tallow per hour; but in 122

grains of tallow there are about 95 grains of carbon, consequently, about fourteen such candles burning together would produce as much carbonic acid in the same time as the Joyce's stove to which I have before alluded.

A spermaceti candle of the same size, will burn in one hour 129 grains of spermaceti; but in 129 grains of spermaceti there are about 100 grains of carbon, consequently, about thirteen such candles burning together will produce in the same time as much carbonic acid as the Joyce's stove.

A stearine candle of the same size will burn in an hour 156 grains of that substance; but in 156 grains of stearine, there are about 121 grains of carbon, consequently, eleven of such candles burning together will produce as much carbonic acid in the same time, as the Joyce's stove.

Another stearine candle from a different maker with a larger wick, but of the same weight, (long four) will burn 175 grains in an hour; but in 175 grains of stearine there are about 136 grains of carbon, consequently, between nine and ten of such candles burning together, will produce as much carbonic acid in the same time, as the Joyce's stove.

Coal gas of average quality, I have found to produce by burning 0·6 of its bulk of carbonic acid; an ordinary coal gas burner on the Argand's principle, having fifteen holes, will consume 5 cubic feet of gas per hour; six tenths of five are three, therefore, three cubic feet of carbonic acid would result from one such light, consequently, two such gas lights burning together, will produce exactly the same quantity of carbonic acid as the Joyce's stove.

But, independently of the formation of carbonic acid, all the common combustibles last named, contain such excess of hydrogen as tends to the further deterioration of the air, by the abstraction of an additional portion of its oxygen, so as to leave an excess of residuary nitrogen, which, of itself, is nearly as deleterious as carbonic acid; the air, therefore, which issues from the glasses of Argand, oil, or gas lamps, or from the flames of candles, will, if received into a proper vessel, by which the entire products of combustion may be collected, prove equally, if not more, deleterious to animal life, than that which results from the combustion of an equivalent quantity of charcoal.

With a view to determine the amount of deterioration the air underwent in crowded assemblies, I obtained some air from a chapel in my neighbourhood, towards the close of the evening service, and, on examination in the ordinary way, it was found to contain a little more than one and a half per cent of carbonic acid. In another instance, I collected some

air from the gallery of a crowded theatre, at eleven o'clock in the evening, about four hours after the commencement of the performances, and this I have found to contain about three per cent. of carbonic acid.

The advantage which I conceive Joyce's stove to possess over the ordinary methods of burning charcoal for warming apartments, is the perfect control over the rate of combustion of the fuel; for while, in a common chafing dish or brasier, almost an unlimited quantity of charcoal may be consumed in a comparatively short space of time, and liberate very suddenly a large volume of carbonic acid, which might be prejudicial to health, if not absolutely dangerous; in these stoves, by their peculiar construction and arrangement of proper sized apertures, the fuel can be consumed only at a certain given rate; and if they be properly adjusted to the size of the apartment they are intended to heat, my experience leads me to believe that no injurious consequences can arise from their employment.

*82, Blackfriars Road, London,* JOHN T. COOPER.
*June* 14*th*, 1838.

Having been present at the experiments made at Mr. Cooper's house, with a view of determining the degree of deterioration which the air suffers by the employment of Joyce's stoves in close rooms, and having examined, in conjunction with him, the composition of the atmosphere under such circumstances, I can certify, that after burning for twelve hours in a close room of the dimensions above stated, that less than one per cent of carbonic acid was, in all cases, found in the air of the room; that such proportion of carbonic acid cannot be considered as deleterious, or in the least degree dangerous, in reference to respiration; that it falls short of the relative quantity of carbonic acid found in crowded and illuminated rooms, or in buildings in which many persons are congregated, such as churches, theatres, and assembly rooms, in which ventilation is generally imperfect, and in which, as far as my experience goes, the relative proportion of carbonic acid always considerably exceeds one per cent. I am therefore of opinion that the said stoves, which are so constructed as to consume only a limited quantity of pure charcoal in a given time, may be employed with perfect security, for all the purposes for which they have been proposed, and I consider the grounds of this opinion sufficiently detailed by the experiments above given.

*London, June* 14*th*, 1838. WILLIAM T. BRANDE.

To Mr. William Harper,
58, King William Street, London Bridge.

## X. Joyces' *Stove Fuel.*

### *Observations of* M. Gay Lussac, *on a new process of fuel, imported from England.**

This process has been much spoken of as something wonderful; with 50 or 60 centimes of properly prepared charcoal, it is said, we may heat an immense room, and keep it at an agreeable temperature for four and twenty hours. Besides, the carbonic acid produced by the combustion does not affect the room, it being retained by the carbonate of soda with which the coal is impregnated, and asphyxia need no longer be feared with this new mode of heat. It has received the assent of the English philosophers, and has also been presented to the "Académie des Sciences."

This much boasted contrivance appeared to me worthy of an examination. I have undertaken it, and in publishing the result thereof, I consider I shall be serving the interests of the public and those of the importers, men of too good faith not to desire to be better enlightened than they have been, on the advantages and disadvantages of this process of generating heat. I may say I feel it fulfilling a duty.

The combustible made use of is a very light charcoal, impregnated, it is said, with carbonate of soda, to retain the carbonic acid produced in its combustion. I obtained a sample of it, and found that it really contained carbonate of soda, or rather, carbonate of potassá; but its quantity is so small that I am convinced it does not amount to the quarter of a thousandth part the weight of the charcoal. Also that it burns with very great facility like charcoal of very light woods.

Hence it is evident that this charcoal gives out, while burning in a room, the same quantity of carbonic acid as an equal weight of any other charcoal; that it infects the air in the same manner, and would produce the same accidents. It is also equally evident that it does not produce more heat than common charcoal, since in the same weight it contains the same quantity of combustible matter.

But being assisted by a proof on the combustion of the new charcoal, I discovered with other assistants, that it was not accompanied with any disagreeable smell, and I thought the small quantity of alkaline salt I supposed had been added to it, might be the cause of the total absence of smell. This is a real improvement introduced into domestic heating, a true

* From the Comptes Rendu des Séances de l'Académie des Sciences, April 9, 1838. Translated by Mr. J. H. Lang.

discovery. This thought was easily submitted to the proof of experiment.

I first ascertained that common charcoal was almost as alkaline as that used in the new process. But to make the experiment more conclusive, I wetted the charcoal with some water slightly charged with carbonate of soda, so that it appeared more alkaline than the English; it was then dried on a stove. Two fires, one fed with prepared, and the other with common charcoal, presented no perceptible difference as to smell. Several similar experiments, changing the proportion of the carbonate of soda, always gave the same result.

Convinced that the salt had no effect in the combustion of the charcoal, I thought the absence of smell, I had observed in the combustion of the English charcoal, belonged to its own nature, for we are aware, that for brasiers, it is not indifferent to employ any sort of charcoal. Having discovered that the English charcoal was very light and certainly produced from some white wood, I caused some pieces of fir planks which had fallen into my hands, to be carbonized. The charcoal thus obtained was found much more sensibly alkaline than the English. Burnt in comparison with common charcoal, it was more agreeable and seemed to resemble the English, but without being able to make an exact comparison for want of a sufficient quantity of the latter.

The importers of the new process of generating heat burn the charcoal in an elegant apparatus which it would be useless here to describe. It will be sufficient to say that it is a real brasier, giving out all the products of the combustion, to the room in which it is placed. And in this consists the great economy of the combustible spoken of; this we cannot deny, it is too well known; but we must not forget that it is only to be obtained by vitiating the air of the apartment, and perhaps injuring the respiration, particularly of inexperienced persons who surrender themselves to too-blind a security.

Be it understood, that the object of our observations is not to proscribe the new process of heating, but only to make it better appreciated than it has been, and to reduce it to its just value. These observations lead us to believe, first, that the combustible is only a well prepared charcoal of light wood, containing no other alkaline salt than that which it naturally possesses; second, that this combustible gives no more heat than any other wood charcoal; third, that the mode of heating employed, which consists in giving out all the products of the combustion to the apartment in which it operates, is a real economy over every other process, but that it is only by vitiating the air and injuring the respiration; fourth, that a well

constructed stove, by the air of the apartment, may render useful about nine-tenths of the whole heat produced by the combustion, without vitiating the air, causing any smell, or affecting the respiration, and that its use is more certain and almost as economical.*

*Remarks of M. Thenard on the occasion of the preceding communication.*

I shall only add a few remarks on what M. Gay Lussac has just said.

It is probable that the charcoal would cause no smell if it had been suitably calcined.

Such in fact is the small coal, and even the charcoal prepared in close vessels, when it has been carried to a high enough temperature.

The apparatus in question may be compared, for effect, to a *brasier* the combustion of whose fuel would be extremely slow, or to one of these common foot stoves which gives its heat for 12 or 15 hours.

The foot stove is filled with charcoal dust and ignited at the surface with a little lighted turf. The combustion operates by degrees; it is kept up by raising from time to time the lower beds with an iron plate; it thus continues from the morning to a late hour at night.

---

## XI. *On the electro-chemical theory in general, and Becquerel's electro-chemical theory in particular, and the relation of electro-motive power to affinity.* By Professor Pfaff†

Electro-chemistry, or the deriving of chemical appearances from the activity of electric powers, has grown forth from the ground of galvanism, embracing within its compass during its gradual development a peculiar form of chemical appearances, determined by peculiar conditions; which representation forms the chemical theory of galvanism.

The human mind, by a natural necessity, endeavours to bring the variety of phenomena under the unity of a principle, and in the same way as the genius of a Newton had won such a

* This information is exceedingly important, and cannot be too strongly impressed on the minds of all those who are desirous of burning charcoal without the risk of its deleterious effects.—Edit.

† Translated from the German.

general principle for distant attractions of masses, however manifold and intricate they might appear, in universal gravitation and its great law; so was it now thought that for the attractions of smallest particles at insensible distances, and for the phenomena of composition and decomposition of bodies dependant therefrom, a similar general principle was found in the electric agency, which appeared to afford, at the same time, a satisfactory explanation of other phenomena which accompany the chemical process, namely, the phenomena of heat and light.

There are, as is well known, four series of facts which have led to the electro-chemical theory, and constitute its proper groundwork.

1. The phenomenon of contact-electricity, received, for at least a long time, by all philosophers, and the order of tension (spannungsreike) under which, in reference to this relation, all perfect conductors of electricity (exciters of the first class) are arranged.

2. The common fact that by the decomposition of a compound body by an electrical current, both the component parts into which the body is separated, whether elements, or themselves binary combinations, possess firm and unchangeable relations in reference to the opposite electricities, so that the one component part is at every time attracted by the positive, the other by the negative electricity; and all bodies form in this respect a long series, which runs parallel with the first, the properly galvanic one, a series in which relatively to the two end members, viz. oxygen, as far as the experiments yet go, as the most negatively, and potash as the most positively electric body, all other bodies are arranged in such an order, that of every two of the row, if separated from each other by an electric stream, as constituent parts of a compound body, the one which stands nearest in the rank to the oxygen passes constantly to the positive pole of a voltaic battery, whilst the other component part, which stands nearest to the positive end, has a tendency to pass to the negative pole. The parallelism between both series, which has become so important for electro-chemistry, and the chemical theory of galvanism, consists, as is well known in this, that of every two bodies of the chemical series, when they disturb the electrical equilibrium by merely mutual contact, that which lies nearest to the potash is relatively positive, that which lies nearest to the oxygen negative electrical, and the electric excitation, and difference of electrical tension, is greater in proportion to the distance which they are removed from each other in that first series.

3. The third fact is, that opposite electricities exercise a powerful attraction on each other; similar electricities, on the contrary, repel each other, and that opposite electricities can, without being united or neutralized, approach each other, even up to apparent contact of the surfaces to which they cling; so that upon separation they appear again with unchanged polarities, and as long as they are in mutual contact are the ground of a strong attraction, and even of an adhesion of those surfaces.

4. The fourth fact, finally, is, that by every compensation of opposite electricities, in the direct proportion of the quantities compensated, and the inverse proportion of the space in which this compensation takes place, an increase of temperature ensues, which advances to a very high degree united with light, and can reach a point which is to be obtained by no other means.

By comparison of the mutual chemical relation of substances with that relation according to which they can be arranged in that definite series, as first and second, it was found, that substances exercise a proportionally stronger chemical attraction, as they are more opposed to each other in the first or second series, or as they stand mutually more remote. A further general law for the reciprocal chemical action of substances was established through induction, the law, that a chemical process takes place only between each two of those bodies, whether simple or compound, which stand in such opposition to each other, as is represented by those series; finally, a still further general law was discovered, that every real chemical process is united with a generation of heat, which is proportionally more active, and advances to red heat and flame, the greater the above mentioned opposition of the two substances is, which enter into the chemical conflict with each other. Now, with this were given also the fundamental features of electro-chemistry, whose problem was only to give the relation of affinity to electricity, which always appeared as its companion, at times openly (in the contact-electricity), at times concealed (in the heat), and to raise this parallelism to a true causal connexion.

This problem has now been solved in different ways, but the very want of agreement amongst chemists in this respect, satisfactorily shows, that we have not yet penetrated sufficiently deep into the nature of that wonderful agent, whose activity in chemical processes cannot be well longer denied, in order to comprehend the phenomena of chemistry in their natural connexion with those more strictly called galvanic.

Becquerel, rightly perceiving the difficulties, which were still left unsolved by the electro-chemical theories of Davy, Berzelius, Ampère, &c., believes that he has advanced nearer to the great goal, by a new modification of these. (Théorie chémique de l'auteur, in the 3d vol. of Traité de l'Electricité et du Magnétisme, p. 406). But even this new theory appears to me equally incapable of supporting a rigid criticism as its predecessors, and I think it conformable to the plan of this work (Revision of the doctrines of galvano-voltaismus &c. by C. H. Pfaff, Professor of medicine and chemistry in the University of Kiel), to make some general remarks on this head, and to add to them a few observations on the relation of electro-chemistry to galvanism.

Becquerel's theory differs essentially from those of his predecessors, e. g. Berzelius and Ampère, in this: that he ascribes neither to the elements, nor bodies compounded of them, as long as they exert no mutual action, peculiar electric atmospheres, charges (Ladungen), or polarities, on which their reciprocal attraction in chemical processes would depend, but supposes that they are surrounded by atmospheres of neutral electricity, so long as they do not enter by the chemical process itself, into a new union. With this neutral electricity, called in Franklin's theory "the natural portion of electricity," stands first, the "power of aggregation," or the attraction of homogeneous particles (molecular attraction) in the nearest relation and dependence, and indeed M. Becquerel finds a proof of this in the fact, that the strongest union of bodies can be destroyed by a sufficiently strong electrical discharge. The same effect would therefore follow, if by means of electric distribution (Elektrische Vertheilung) whose power can be multiplied indefinitely, the two electricities, which were before more closely bound together, were withdrawn from a body, with whose sudden appearance (austritt) and entire loss, a perfect dissolution of the cohesion, would, in a similar way, be united. Here, however, M. Becquerel appears to have overlooked the fact that this withdrawal of the two electricities does not effect the dissolution of the cohesion of the bodies, because this cohesion depends on their presence, but because these electricities, when separated from each other, become active, in proportion to the rapidity with which the separation takes place, by means of their repelling power, and for just this reason work in opposition to the attraction, by which the smallest particles mutually cohere, and effect their separation from each other. An exactly equal quantity of electricity could be withdrawn from the body without the slightest alteration in its cohesion, if this withdrawal took

place only gradually. Let us imagine for a moment all fluid, and other latent heat, of a body, suddenly set free, and spreading out into a cold medium, an entire separation of the particles from each other, and a dispersion of them, by means of a sudden change of the former fluid state into a gaseous one, would certainly accompany this movement and simultaneous loss of the heat. Who would conclude from this that the heat was the cause of the cohesion of the particles, when it much rather works in exact opposition to it?

M. Becquerel has at the same time considered in his theory the phenomena of thermo-electricity; but here also we find assertions which do not appear consistent with each other. He says that when the heat in its movement, in its propagation through bodies, (e. g. through an arc of metal) encounters opposition, it separates itself into its two factors + E and —E, that the + E, which, according to all experience, has a greater power of penetrating an opposing medium than — E, leaps over the resistance, and that in this way a current of (positive) electricity is introduced from the heated parts to the colder ones. Further on, however, excitation of electricity by means of heat is understood by him in quite a different way; the heat being considered not as the source, but only as the *causa efficiens* of the electricities. Namely, in that the heat separates by means of expansion of the particles from each other, it must operate in a similar way with the division of bodies, in consequence of which, as is well known, the divided surfaces appear with opposite electricities. Then, again, should a heated particle attract more + E, and drive forward — E, in this way the process goes forward, as one particle after the other becomes heated; the result of which is a movement of the electric fluid.

He says, that the compensation of opposite electricities does not in itself produce heat; but that this is the effect of the opposition which they experience in their progress and passage through the body. M. Becquerel appears to have kept in view Œrstead's ingenious theory on the relation of heat to the conflict of opposite electricities, and guards himself in the mean time against all abstract metaphysical explanations, p. 414. Nous evitons, comme on le voit, d'emettre des idées abstractes ou métaphysiques, que l'esprit ne saisit que difficilement, et qui ne jettent par conséquent que de la confusion dans la science.

M. Becquerel is not easily stopped by difficulties. By blows, or rubbing, we set free a quantity of opposite electricities, which are combined with the atoms, and these unite themselves again, and produce heat. M. Becquerel appears

to have quite forgotten what he expressly asserts of atoms, those at least of elementary bodies, therefore, also of metals, viz. that, when they are disconnected with atoms of another element, and unite themselves to masses of particles (massentheilchen), they are surrounded neither with atmospheres of + E nor — E separately, but with atmospheres of both +E and— E, the so called natural electricity or 0. Now, we can easily conceive that these electricities are separated by a mechanical action, not indeed to remain separated, but to unite themselves again momentarily. The case was certainly different if binary compounds, oxides for instance, became heated by rubbing or blows, because M. Becquerel supposes in these separated polar atmospheres of + E and — E surrounding the particles of the oxygen and the radical, which stand opposed to each other, in which case one could still imagine some such compensation of opposite atmospheres; but is it not the very metals which admit of being so strongly heated by hammering; and brimstone and charcoal by friction? Where can we here find in the union of opposite electricities, supposed by M. Becquerel, the condition previously supposed by him as essential for this excitation of heat, namely, the retardation of the electric current in its movement, the opposition whereby its rapidity is hindered? Where do the particles of + and — E, which lie close on each other and unite, find such an opposition? Is the idea of streams of opposite electricities at all applicable to this case? The particles also, whose + and — E have united themselves in order to the production of heat, and those which, as we shall see below, are only chemically united by means of these electrical atmospheres, would be compelled to separate, and by means of this mechanical operation, when it produces heat, a decomposition must at least partially follow; a fact for which no experience vouches.

M. Becquerel, in his chemical theory of properly galvanic phenomena, allows himself pre-suppositions which are equally arbitrary. At once, the fundamental fact from which M. Becquerel goes out, appears to be doubtful. He says, that when alkalies and acids act chemically on each other, unite with each other, the positive electricity passes over to the acid, the negative to the alkali. As many particles of each of these substances as become active, so many particular electric streams are set in motion, which cannot pass away over the surfaces of contact without opposition, and by means of their reunion in the fluid produce the increase of temperature. On the contrary, he says that by the decomposition of such an union, the acid receives the negative, the alkali the positive

electricity. In order to make this assertion agree with the one afterwards made by M. Becquerel, we must suppose, that the first electrical excitation according to which the acid becomes positive, and the alkali negative, is in the very first moment of their mutual action, a consequence of the decomposition of their natural fluid, by means of which the particles of the acid remained polarized with negative, the particles of the alkali with positive electricity, for that these particles (and those analogous to them in the binary unions) exist, in the actual union, in this partial electrical state, and that the intimacy of this union depends merely on the grade of this electrical polarization. In this way, according to Becquerel, stand the chemical affinities in the nearest relation with the electrical powers (polarities) of the particles: every thing that augments the latter, augments also the power of cohesion; whatever weakens them, weakens the same. That which characterizes and distinguishes this new theory from the electro-chemical theory of Ampère, Berzelius, &c., consists accordingly in this, that the polar electrical state of the atoms is no state inherent in these; but as it were first called forth by the chemical process in them, just so, perhaps, as by the friction of the surfaces of two heterogeneous bodies, the opposite electricities first free themselves from the natural fluid, and gather together, which at all events makes it easier to conceive, how one and the same element, e. g., chlorine, bromine, &c., can be relatively to the combustible bases, negatively, relatively to the oxygen positively electrical. It is, however, at the same time, clear, that according to this view, the electrical powers are by no means the first agent, the real spring of the chemical process; but pre-suppose the activity of independent affinities, which, first, by their mutual action on each other, call forth, as it were, the electrical polarities, which again in their turn by their reciprocal attraction maintain the cohesion. Exactly according to this view, therefore, the atoms in the first moment of decomposition, or as it is called the *status nascens*, appear surrounded with their partial electrical atmospheres, the acids and the elements corresponding to them (when they separate themselves from the combustible bases) appear with negatively electric, the alkalies and their corresponding elements (the metals of alkalies, of earths, of the fundamental oxides of the heavy metals) with positively electric atmospheres, and at all events it is explained according to this view, how it is that the elements act exactly in this state with the strongest affinity on each other, and under these circumstances form unions, which do not follow when these elements have once

entered into their gaseous state. For the electric polar atoms do not continue in this condition, but quickly attract from the surrounding medium the opposite electricity, the atoms of acids (oxygen, chlorine, &c.) attracting +, the atoms of alkalies (metals, the combustible bases) — E, and veil themselves from others in atmospheres of natural (not polar) electric fluid.

The explanation of the process in the simple galvanic circle rests entirely on this chemical principle. When the zinc is plunged in water, strengthened either with acid or alkali, the metal reacts slowly on the water, and attracts its oxygen, which comes forth, surrounded with its negatively electric atmosphere, from its union with the hydrogen, and in order that the zinc, which is only surrounded with an atmosphere of naturally electric fluid, may be enabled to unite itself with the oxygen, it must surround itself with an atmosphere of positive electricity, which process is facilitated by the zinc parting with its negative electricity to the copper with which it stands in contact in the circle, from which again this negative electricity streams over into the fluid. This process is favoured by the hydrogen, which endeavours to hold back the oxygen, being carried over to the copper by the working of the electric current (par l'effet du courant.)

In order, finally, to explain decomposition by the voltaic pile, M. Becquerel starts from the position, that streams of two opposite electricities must be supposed; for although we are enabled to exhibit either of them singly, and separate from the other, yet that the phenomena of common, as well as voltaic, electricity, show that in one direction (that of the positive stream) a greater power of overcoming intervening opposition exerts itself than in the opposite direction, which is only conceivable under the supposition of two streams from both sides, but of dissimilar quality. If we take as a ground that each of these has an undulating movement (ondulation), which it can impart to electricity of the same kind with itself, and cause it to move in the same way, so would it follow, that the positive undulation (the positive stream), as it passes from the positive to the negative pole, would be communicated to the positively electric atmospheres of the corresponding particles of the compounds (so called electro-positive matter), which would in that case carry over to the same pole with themselves the material particles with which they are intimately united, and deposit them there, because they cannot force themselves into the polar plates or wires. The same thing would hold good of the negative undulation (the negative stream), which would communicate

itself to the negative atmospheres of the particles of the acid, and of the electro-negative elements generally, by which means these latter would be borne along with them to the positive pole, and deposited there.

If the assertion was correct which I put forward after Davy's experiments (Physic. Dictionary, Vol. IV., sec. 2, p. 801) that, when an actual chemical union of elementary bodies, which are electrically opposed to each other in the contact, takes place, every trace of electricity vanishes, we should be relieved from all further trouble of reasoning against this, or any other purely chemical theory of galvanism. But since this, experiments have been made known, which appear directly to prove that when the chemical process actually takes place and the opposed elements have united themselves with each other, that evident signs of opposite electricity are to be observed. I allude to the experiments of a very trustworthy investigator; but which, as far as I know, have not yet been confirmed by those of any other philosopher. Those of Pouillet, who by the aid of the condensor has shown that by the burning of charcoal, hydrogen, and the compounds of carbon and hydrogen (spirit of wine, ether, the fixed oils), positive electricity accompanies the product of the combustion, and therefore the oxygen, whilst the combustible body, as far as it has not yet entered into the union, receives negative electricity (Poggendorf's Annals, Vol II. p. 417). Besides this there are some experiments by Nobili, which prove that by an union of acids with bases, by the dissolving salts in acid, electrical currents are produced, which (according to the supposition of one electric matter, or according to the dualistic theory the positive) take a direction from the alkali to the acid (Pogg. Ann. Vol. XIV. p. 157.) In Nobili's experiments every co-operation of the electro-motive action between the metals and the fluids was excluded. Two perfectly homogeneous platina plates were plunged, namely, into similarly saturated solutions of saltpetre, and, consequently, the electro-motive action was equal on both sides, and necessarily preserved the equilibrium, so that no current could take place from this quarter. Into the glasses, in which the solution of saltpetre was, were plunged cotton (or asbestos) threads, also moistened with solution of saltpetre. One of the glasses communicated by one of these threads with a vessel containing nitric acid, at one end of the other thread, whose other end was plunged into the glass with the saltpetre solution, was a piece of caustic potash, which in the moment of its contact with the nitric acid produced an electric stream which passed from the alkali to the acid. Other acids

acted in the same way as the nitric, and alkaline earth as the alkali for example, lime and carbonate of lime.

In the mean time a question remains in both these cases. Was the excitement of electricity a consequence of the actual chemical union, did it first take place with this union, or *did it not rather precede it*, was it not even in these instances an operation of the contact, by which every chemical process begins, of that first touching which must surely be distinguished from the actual intimate union, which necessarily precedes every chemical conflict, and is inseparable from it? Here now lies the great question of dispute between the, as it were, pure orthodox voltaic theory, and a combination of this with the chemical theory.

Immovably firm stands the fact that merely mutual contact of heterogeneous solid bodies suffices, without the co-operation of a chemical process, to the excitation of electricity, and equally established is the fact, that this electric excitement, by means of mere contact, far surpasses in strength that which could possibly be ascribed to a chemical action, simultaneously accompanying the contact. Let us only compare the electricity which a piece of zinc, held in dry fingers, communicates to a copper condenser, with that which a piece of copper, attacked most sharply by nitric acid, communicates to the same condenser; the first surpasses the last twentyfold. Indisputable also appears to me the fact, that in a common galvanic circle the electric stream is produced and supported by the electromotive action of the two metals which are in contact, and, at least not primarily and immediately, by the chemical action of the fluid on the metal (the zinc). For if we substitute for the ordinary zinc plate an amalgamated one, on which the acid solution, as far as itself is concerned, even as Faraday allows, has no chemical action worth mentioning, so far from the battery having lost power, it exhibits much greater strength, exactly agreeable to the principle that the current depends on the electro-motive action of the metals on each other, since, according to the experiments with the condenser, amalgamated zinc exhibits a much greater difference of electrical tension with the copper, than common zinc with the copper. That Becquerel's above mentioned theory of the simple galvanic circle rests on arbitrary pre-suppositions, is self-evident. That quality of the zinc, by which it first surrounds itself with a positive electric atmosphere, in order to be able to unite itself with the negative acid, is a true *qualitas occulta*, and pre-supposes, as it were, a feeling in the zinc and a consequent desire, and an action altering itself according to circumstances. It is also not at all to be understood, how, in the way mentioned, an electrical current can at all arise, by whose action

the hydrogen can be drawn away from the oxygen, and carried over to the copper. For let us imagine for a moment the isolated atom of hydrogen with its positive electrical atmosphere; the latter can in no way cause a movement in any direction. This would be assuming with Faraday and Grotthuss, that the affinity acts only with reference to the oxygen of the neighbouring particles, which would, however, be something very different from an electrical current.

If electro-chemistry suffices in no case, with the polar electric powers ALONE, perfectly to explain the phenomena, but always has, as it were, still in reserve a power of affinity in the material particles themselves, which is indeed first necessary in order to hold the electric atmospheres round the atoms, so perhaps ought we not entirely to reject the view, which seeks the electro-motive power in this affinity itself. The first appearance of its commencing activity would be the disturbance of the electrical equilibrium, or the setting free the electricities, which might be united with the atoms, either as preponderating positive or negative electricity. As long as an actual chemical union does not take place, so long are the affinities of the material particles not yet perfectly united; they act still electro-motively, and are in a state of free electric tension. If the chemical conflict attains its object, these affinities enter into perfect mutual union; the electricities, which are now no longer held separate from each other, strike as it were together; the free tension ceases; and warmth and light take its place. The results of Pouillet and Nobili are only the effects of the contact which precedes the actual union of the affinities, as long as they yet act only electro-motively, and which, either through flowing, or aggregation in the condenser, are withdrawn from the immediate compensation. According to this view the relatively electro-positive as well as electro-negative elements would stand in a double opposition, in one towards each other, and in one towards the opposite electricities, and the process of chemical composition and decomposition would be the result partly of the preponderancy of the mutual affinity of the material atoms over their affinity to the opposite electricities, partly of the preponderancy.

---

## XII. *On a water cushion for electrical machines, by* Mr. C. V. WALKER, *in a letter to the Editor.*

Kennington Grammar School,
Sir, June 4, 1838.

In common with other electricians, I have felt the inconvenience arising from the want of uniformity in the shape of

glass cylinders for the electrical machine. Chancing to have one, which was very irregular, and finding the inefficiency of the common cushion, (for while at times it exerted a strong pressure, at other times it exerted no pressure at all) I was led to adopt the arrangement, of which fig. 10, Plate I, is a section, and fig. 11, a back view. A B C is a cushion formed of Indian rubber, and covered with wash-leather, presenting to the cylinder a surface A B C. It is filled with water, by means of a condensing syringe, through a valve D, at the back. To give strength, the side A C is attached to a metal plate *x y*, by passing a needle, with stout thread, through the leather cover and through holes drilled in the edge of the plate, as seen in fig. 11.

The amalgam is not applied to the cushion itself; but to a silk flap attached to its lower edge, and passing between it and the cylinder.

The free motion of the particles of water among themselves rendered this a great acquisition, for it accommodates itself so as always to present the requisite pressure to the revolving cylinder. Should any of your readers object to it on account of waters being present, I would say, that the experience of two years satisfies me that no dampness originates from that source; and this from the Indian rubber being water-proof, and the valve water-tight.

The merit of inventing the water cushion is due to Mr. T. Forster, 14, Basing Lane, who applied it to a purpose in surgery: but, on my suggesting that it would be a desirable substitute in place of the usual rubber of the machine, he kindly arranged one as above, which answers every expectation.

I remain, Sir,

Your obedient servant,

*To W. Sturgeon, Esq.* CHARLES V. WALKER.

---

## XIII. *Mr. Fox's Experiments.* By J. Murray, Esq. F.S.A. F.L.S. F.H.S. F.G.S. *&c.*

### *First Letter to the Editor of the Annals of Electricity.*

Sir,

Electricity is a fine field wherein genius may well revel. I only wish that I could find time to be an occasional contributor to the annals of a science I love so well.

Among the many active experimenters that share in its pleasures, I do not know one whose results promise to shed more light on some of the more recondite phenomena of geo-

logy, than the curious and interesting experiments of Mr. R. W. Fox. I think his theory of metallic veins extremely beautiful and highly satisfactory. There is some difficulty, I admit, not in reference to the opening of the fissures, but of maintaining them in that state: the walls having a tendency to close in upon the miner. The progressive widening of the fissures from time to time, however, certainly obviates much of the difficulty. The natural machinery employed by a prospective Providence in replenishing these veins with their metallic contents is at once simple and sublime.

I have no doubt whatever that Mr. Henwood is a clever man, but certainly all those who, with myself, have the pleasure of knowing Mr. Fox and have witnessed the progress of his experiments, will be firmly persuaded, from the accurate electro-chemical knowledge he possesses, that he is not likely to offend in any of the conditions necessary to sustain the character of an acute observer. The specimens he was so good as to present to me, certainly indicate the definite changes he has so well described.

But the most curious of all Mr. Fox's experiments is unquestionably the *lamination* of clay. It illuminates the geology of rocks, and shows the all-pervading power of this subtile and still mysterious agent, and its connexion, perhaps, with the lamellar form of *gneiss* and its congeners; and of *mica-slate* and *clay-slate* and its subordinate shale. Perhaps, however, one of the most interesting of its associations may eventually be found to be *coal* and many of its phenomena, as to their solution, remaining apocryphal or undetermined. Lord Willoughby's interesting experiments on the *compression* of peat, and the production of an imperfect coal, conjoined with electric influence, may contain the elements of the solution of its formations, and reduce to its proper and legitimate dimensions, that indefinite and mighty measure of *time* deemed necessary for the production of coal, and in the expenditure of which element, modern geologists are so lavish and unlimited.

I remain, Sir

Your obedient humble servant,

London, 11th June, 1838. J. MURRAY.

---

*Lightning Rods.* By J. Murray. Esq. F. S. A. F. L. S. F. H. S. F. G. S. *&c.*

*Second Letter to the Editor of the Annals of Electricity.*

Sir,

While no reasonable doubt can be entertained of the sterling talent of Mr. Harris as an eminent electrician, I must confess that I think Mr. Roberts's suggestions justly entitled to deep consideration.

I confess that my views of Mr. Harris's conductors, as applied to the masts of ships at sea, are something similar to those of your esteemed correspondent. No doubt Mr. Roberts is well acquainted with M. Gay Lussac's proposed plan for a lightning rod, namely, a metallic rope composed of several strands of twisted wire.

I believe, Sir, you have stated that the best conductors are not free from the danger attendant on what has been called "the lateral explosion." I have always considered this phenomenon to be connected with the principles of induction. It is quite true, however, that a very formidable electric discharge may be transmitted through the more highly inflammable materials, by means of a *good* conducting medium, yet without ignition, provided the conductor be continuous or uninterrupted.

I will frankly confess, as the result of my numerous observations on the effects of lightning, and experimental reseaches on electricity, that I for one think it pervades the surface chiefly in the case of *good* conductors, as of silver and copper; and that its depth, except when such surface is obscured by oxide, is scarcely appreciable. I have little doubt, therefore, that *superficies* is *chiefly* concerned, and ought not to be overlooked in the estimate; and, if I mistake not, this synchronises with the views of M. Biot in his Traité de Physique.

The interesting subject of conductors has engaged much of my attention; and as my lightning rods have been extensively erected, I shall be obliged to you to allow me to submit for the consideration of your readers and the contributors to the Annals, a brief description of their construction and the principles on which they are founded: premising that in their application to the masts of vessels, I recommend flexible and sliding tubes like those of a telescope, a provision readily adjusted, to the case of the top-mast or top-gallant-mast, when struck in a storm.

A point for the safe receipt of the lightning, a good conducting material for its transmission; a sufficient surface &c. for quantity; a continuous unbroken channel from the summit to the humid subsoil, and the preservation of its conducting character unimpared, and in perpetuity; these seem to embrace all the desiderata that should enter into the composition of a perfect conductor, and I believe none of these requisites will be found wanting in my lightning rod.

My conductor, then, consists of a series of *hollow* copper tubes, composed of gas piping and rendered continuous by connecting joints at definite lengths. This terminates in a solid pyramid of copper at top, screwed into the pipe, and surmounting the loftiest part of the building. The *rod* thus

presents a fine point and four sharp angles for the reception of the meteor. Beneath this pyramidal termination, the stem of the conductor is perforated, so that the electricity may escape to the earth by the *internal* as well as the *external* surface. The lower part of the pipe enters the earth inclined at a certain angle from the vertical wall, and terminates in the wet subsoil.

The surface of the conductor is preserved unimpaired on galvanic principles, namely, by ribbons of zinc, at intervals on the stem straps of leather being interposed, and copper wire forming the circle of communication. Loops of zinc wire connect the conductor to nails driven into the wall.

I am, sir,
Your obedient servant,
J. MURRAY.

*London,*
*June* 11*th*, 1838.
*To Mr. W. Sturgeon.*

---

## XIV. *Description of a new Bell Machine used in Pneumatic experiments, invented by* Mr. Henry Castell.

Portsmouth.

Having frequently heard complaints made of the inefficiency of the bell machines now used in pneumatic experiments, I have endeavoured to improve them; and have invented two self-acting ones which have been found to be far better than those generally used. Fig. 20, Plate III, represents the machine best adapted for general use, as it may be manufactured at a much lower price than fig. 21; and it will be generally found sufficient for the experiments it is required for. It will ring the bell, when wound up, between six and seven minutes, which is I believe a sufficient time to exhaust a common receiver and allow the air to return and demonstrate that the bell will then sound. The bell, from its being placed over the wheels may be much larger than those now used, and of course will sound more distinctly, which will prevent the working of the machine being heard, and the hammers working outside of it, their motion will be seen, which cannot be when acting under it. The revolving colours may be applied to the use of the escapement wheel C, as shown in fig. 23, H; and the machine may be attached to the top of the receiver by a cord attached to the interior of the bottom of the bell. The machine fig. 21, is on a larger scale than fig. 20, and a different plan. It will continue working upwards of half an hour; it is capable of striking a bell that would be distinctly heard by a very numerous audience, and there

would be a short interval between the blows of the hammer, which would render the sound rather more distinct; and the two opposite colours H, revolving at a sufficiently slow rate to prevent their blending, would show a long distance from the machine that it was in motion, and may therefore be preferred by gentlemen lecturing at Institutions or Public Halls.

### *Description of the Plate.*

Fig. 20, Plate III.

A. The square to wind up the spring.
B. The barrel with 42 teeth containing the spring.
C. The escapement wheel with 24 teeth.
D. The verge to act on the teeth of the wheel C.
E. The hammers moved by the action of the escapement to strike the bell.
F. The bell.
G. The revolving colours may be applied to the end of the escapement wheel pinion, as fig. 23, H.

Fig. 21.

A. The barrel with 78 teeth containing the spring.
B. Pin wheel 64 teeth, pinion 8.
C. Stop wheel 56 teeth, pinion 8.
D. Forth wheel 44 teeth, pinion 7.
E. Fly, pinion 7.

Fig. 22, represents the mechanism complete.

F. The bell.
G. The hammer to strike the bell.

Fig. 23.

N. The square to wind up the spring.
H. The revolving colours to show the machine is in motion.
I. The spring lever to stop the machine.
K. Pin for the lever I to work on when its position is altered.

Fig. 24, shows the action of the hammer.

L. The hammer spring.
M. Hammer arbour and tail.
B. The wheel raising the hammer tail.

---

## XV. *London Electrical Society.*

*Tuesday, 29th May,* 1838.—The first annual meeting of the members of this society was held in the Theatre of the Royal Gallery of Practical Science, Adelaide Street, to receive the

report of the committee, and on general business; J. E. Johnson, Esq. M.A. in the chair.

The report, which was read by the assistant secretary, congratulates the society on its present prospects, and expresses the confidence of the committee that the same steady pursuit of science, and avoidance of all mere personal interests; the same unanimity and anxious desire of assisting others, without in any way (as a body) obtruding their own opinions, will secure to the society still further support.

The thanks of the society were voted to the honorary and assistant secretaries, and also to the committee, with a request that they would continue their exertions until a permanent council was formed.

*Saturday, 2d June.*—A paper by C. Griffin, Esq. of Leamington, on homogeneous attractions of electricity, was read. This paper is given at length in the present number of the Annals, page 36. A paper was also read by Mr. C. V. Walker, on the local attraction of the magnetic needle on board of steam boats. This paper is also inserted in the present number of the Annals.

It was then unanimously resolved, that the future evening meetings of the society should take place on the first and third Tuesday of every month, instead of Saturday.

*Tuesday, 19th June.*—A paper was read by Mr. Jeffery, containing observations on some new facts in voltaic electricity. The observations made by the author of this paper arose from the circumstance of Mr. Clarke, Philosophical Instrument Maker, of the Lowther Arcade, having some time ago combined a number of Zamboni's piles by means of quicksilver connexions, for which purpose short terminal wires were attached to the extremities of the piles and amalgamated; in concluding the experiments the piles were severally folded in brown paper. When examined twelve months afterwards, the terminal wires exhibited the following results:—the amalgamated terminals were covered with minute metallic crystals, which the author thought were propuced by the electric action of the piles to which the wires had been attached.

A paper was also read by Andrew Crosse, Esq., of Bromfield, Somerset, containing an account of a series of daily observations made with a sustaining battery, to ascertain the increase or diminution of the power of the same, as corresponding with the increase or diminution of the temperature of the atmosphere during a part of the last winter, and commenced previously to the very severe frost; and also a few remarks on the agency of heat, in electro-crystallization. As it is the intention of the society to print this paper in their transactions, we can only briefly allude to the contents.

The observations on the sustaining battery are for a period of 28 days, from the 23d December, 1837, to 19th January, 1838. The weekly average result is as follows:—

| Gas collected.* | | Average Temperature. | |
|---|---|---|---|
| First Week | 434° | a little above | 50° |
| Second Week | 388° | not quite | 46° |
| Third Week | 310½° | ,, | 37° |
| Fourth Week | 306° | a little above | 32° |

There is, however, a singular fact connected with the above observations; viz. that on the last day, with the thermometer at 32° with ice in all the cells, the quantity of gas obtained in the volameter was exactly the same as on the first day, when the thermometer was 50°.

In the experiments for the formation of crystals, Mr. Crosse tried the effects of heat in combination with voltaic electricity. The solutions were kept at the boiling point from one to six weeks, the solutions being constantly replaced as they evaporated, which in some instances exceeded seven gallons in every twenty-four hours.

Mr. Crosse exhibited several specimens of the crystals, chiefly of copper and its compounds. Taking up one Mr. Crosse said, this is iridiscent copper-ore, produced in eight months by this simple process. When the negative and positive cells contain sulphate of copper and muriate of ammonia, crystals of various kinds are formed. If the process of crystallization be carried on in the dark, with hot solutions and water, there will not be any cessation of action: but in the usual way of experimenting, there seems to be a point at which crystallization stops. He produced a specimen (formed in the absence of light) of red oxide of copper in crystals perfectly octohedral, which he considers to be very rare in nature. Mr. Crosse stated that six sided prisms of carbonate of lime, attached to a coil of wire suspended in a glass vessel, were destroyed when exposed to the light.

Mr. Crosse is inclined to think, from a series of experiments which he is at present making, that fissures in the earth are caused by electrical action, and that in all probability every description of gems, found in the earth, can be formed by the union of pressure, heat, electricity, and absence of light.

* Mr. Crosse collected the liberated gases in a cylindrical glass tube, graduated into equal parts, which are here called degrees.

## XVI. *Proceedings of the Royal Irish Academy.*

(Extract.)

April 9, 1838.

SIR WM. R. HAMILTON, A. M., President, in the Chair.

Dr. Kane read a paper on the sulphates and nitrates of mercury, particularly the basic salts formed by ammonia.

In a former paper Dr. Kane had shown, that by the action of ammonia on the chlorides of mercury, there were generated compounds involving the radical $NH_2$, (amidogen,) and the design of the present paper was to develop the function of the ammoniacal element of the oxyen salts of that metal. It was found, however, that from the diversity of the results of former chemists regarding the common basic salts of mercury, it became necessary to re-examine them in order to establish some fixed points to which the constitution of the ammoniacal bodies might be referred.

Among the numerous formulæ which have been assigned to Turbith mineral, Dr. Kane has found $Hg\,O.\,SO_3+2Hg\,O$ to be correct, ($Hg=101,4$,) and there is no other subsulphate; the pure turbith, no matter how prepared, possessing that constitution. By the action of ammonia on persulphate, or on turbith mineral, there is generated a white powder, which was submitted to most careful analysis, and gave the formula $HgO.\,SO_3+2HgO+Hg.\,NH_2$, was found. The proof that the fourth atom of mercury in this body is not an oxide, is full and positive, and hence Dr. Kane no longer retains the parallelism of theories used in his former memoir, as the absence of oxygen may now be considered as fully proved.

The crystallizable nitrate of the red oxide of mercury has been found $NO_5+2Hg\,O+2HO$, as the younger Mitscherslich had stated; and Dr. Kane shows that there are two basic pernitrates, of which the one, yellow, is similar in composition to the basic nitrates of copper and bismuth, that is, $HO.NO_5+3Hg\,O$, and the second, of a brick red colour, he is disposed to consider as $NO_5+6Hg\,O$, though it was found exceedingly difficult to decide whether the compound did not retain a trace of water.

It is known that for the composition of the white precipitate, which is given by ammonia with pernitrate of mercury, different results had been obtained by Mitscherslich and Soubeiran. These discrepancies have been reconciled by the discovery that there are at least two, perhaps three, precipitates, almost identical in colour and properties, produced in

this reaction, but which differ remarkably in their chemical constitution. When the solutions are cold, and the ammonia not in excess, the white precipitate has the composition $NH_3.NO_5+3Hgo$, the formula obtained by George Mitscherslich, but if the liquor be warmed, it becomes $Hg.O.NO_5+2Hgo+HgNH_2$. This modification is evidently that which was analyzed by Soubeiran, for he found one atom of acid, one of ammonia, and four of mercury, which ratio is quite true. It will be at once seen that the former body corresponds to the yellow basic nitrate, the oxide of hydrogen being replaced by the amide of hydrogen. Thus

$$\begin{array}{ll} NH_3 & = NH_2.H = Ad H. \\ HO.NO_5 & + 3Hgo. \\ HAd.NO_5 & + 3Hgo. \end{array}$$

This view is remarkably corroborated by the fact, that when a solution of nitrate of ammonia is poured on the yellow basic nitrate, the white powder is formed, while nitric acid is set free. Another remarkable case of combination is shown by comparing the powder formed by boiling the white precipitate with two of those described in this paper. Thus there are

$$\begin{array}{l} Hg.cl + 2Hgo + HgAd. \\ Hg.SO_4 + 2Hgo + HgAd. \\ Hg.NO_6 + 2Hgo + HgAd. \end{array}$$

To which may be added, the oxychloride

$$Hg.cl + 2Hgo + Hgo.$$

When the white ammonia subnitrate is boiled with solution of ammonia, it dissolves, and a crystalline substance is deposited of a very interesting nature. Its formula, by analysis, is

$$3\,(NH_4O.NO_5) + 4Hgo,$$

But the circumstances under which it is formed rather indicate for its rational formula the following:

$$(HgNO_6+2Hgo+HgAd)+2(NH_4O.NO_5)+2HO$$

George Mitscherlich had already obtained a compound of a similar nature, being

$$NH_3NO_5+2Hgo=(HgNO_6+2Hgo+HgAd)+NH_4O.NO_5.$$

Passing to the nitrates of the black oxide of mercury, Professor Kane has verified the analysis of George Mitscherslich, of the two crystallized protonitrates; he then shows that there exists one definite subnitrate of the black oxide, the yellow powder analyzed by Grouvelle. Dr. Kane finds that this powder always retains an equivalent of water, that its formula is $HO.NO_5+2Hg_2.O$, and that the grey subnitrates

which have been noticed by some chemists, are impure mixtures of black oxide and the yellow powder. Dr. Kane considers the nitrates of the black oxide of mercury to be thus related:

First crystallized, $= \text{HG}_2.\text{O}.\text{NO}_5 + 2\text{HO}.$
Yellow powder, $= \text{HO}.\text{NO}_5 \quad + 2\text{HG}._2.\text{O}.$

Second crystallized, $= 2\text{NO}_5 + 3\text{HG}._2.\text{O} + 3\text{HO}.$ A double salt.

Great difficulty was found in determining what specimen of Hahneman's mercury should be considered as pure and fit for analysis. Considering that the most important sources of error tend to throw the value of mercury too high, Dr. Kane derives his formula from the lowest number which he obtained by analysis, and these numbers were given always by the blackest and purest looking portions. He finds, on these grounds, for the ammonia subnitrate of the black oxide, the formula $\text{NH}_3.\text{NO}_5 + 2\text{HG}._2.\text{O}.$ which is related to the water subnitrate in a similar manner to what holds in the corresponding compounds of the red oxide.

Thus, in this paper, two propositions are developed: 1st. Increased evidence of the formation of metallic amides. 2d. That ammonia as amide of hydrogen is capable of replacing oxide of hydrogen in its various functions in the qnicksilver salts.

---

Professor Apjohn read a paper "on the Properties of a new Voltaic Combination," by Thomas Andrews M. D., Professor of Chemistry in the Belfast Institution.

The object of the author in this paper is to extend the results which he has already obtained on the influence of voltaic circles upon the solution of the metals in nitric acid to the case of concentrated sulphuric acid. When a plate of zinc is heated to the temperature of 240° cent. in sulphuric acid, of the sp. gr. 1.847, it is dissolved with the rapid disengagement of a mixture of hydrogen and sulphurous acid gas; but when a similar plate, voltaically associated with a platina wire, is introduced into the same acid, its rate of solution is reduced to one-third of the other, no gas appears at the zinc, and sulphurous acid, almost perfectly pure, separates at the platina wire. Similar effects occur at other temperatures, but the proportion between the quantity of zinc dissolved when alone, and when connected with platina, varies with the temperature. A minute investigation is given of the effect of the distance between the metallic surfaces, and of their relative extent upon the solution of the zinc, and the development of the electrical

current; from which it appears, that, as in common cases, the action on the zinc was increased by diminishing the distance between the zinc and platina in the liquid, but on the contrary, was diminished by increasing the extent of the platina surface. The latter anomalous result is carefully examined and explained.

The influence of the contact of platina with the other metals, resembles, in general, its effect upon zinc, except in the cases of mercury and arsenic, in which the solution does not appear to be retarded in this way, nor is there almost any gas evolved from the platina.

The general conclusion drawn by the author from all his experiments is, that the formation of a voltaic circle generally diminishes, and never increases chemical action, when the liquid conductor is an oxy-acid of such a strength, that the electro-positive metal is oxidized from the decompositions, not of water, but of the acid itself.

---

Professor Mac Cullagh exhibited and described a new optical instrument, intended chiefly for the purpose of making experiments on the light reflected by metals. The instrument consists of two hollow arms or tubes, moveable about the centre, and in the plane, of a large divided circle, each arm being provided with a Nicol's eye-piece, or some equivalent contrivance for polarizing light in a single plane; while in one arm, which is of course crooked, a Fresnel's rhomb is interposed between the eye-piece and the centre of the circle. At this centre is placed a stage for carrying the reflector, with its plane perpendicular to the plane of the circle, and having a motion to and fro for adjustment. Each eye-piece, as well as the Fresnel's rhomb, turns freely about the axis of the arm to which it belongs, and is provided with a small circle for measuring its angle of rotation. When the two arms are set at equal angles with the reflector, and the observer looks through the crooked arm, he will see a light admitted through the straight one; and then, by turning the Fresnel's rhomb, and the eye-piece next his eye, he will be able, by means of their combined movements, to find a position in which the light will entirely disappear. An observation will then have been made; for the light, before its incidence on the metal, is polarized in a given plane by the first eye-piece; but after reflexion from the metal, (as we know from Sir David Brewster's experiments,) it is elliptically polarized; and our object is to determine the position and species of the little ellipse in which the reflected vibration is supposed to be performed.

Now, the axes of this ellipse are parallel and perpendicular to the principal plane of the rhomb, when it is in the situation above described, where the light completely disappears: and the ratio of the axes is the tangent of the angle which that plane makes with the principal section of the eye-piece next the eye. The angles are read off from the divided circles; and thus, for any angle of incidence, and any plane of primitive polarization, we can at once ascertain the nature of the reflected elliptic vibration. Professor Mac Cullagh mentioned that the instrument was made last year with the view of testing certain formulæ which he has proposed for the case of metallic reflexion, and which have been printed in vol. xviii. pp. 70,71, of the Transactions of the Academy but that he had not yet found leisure to make the various adjustments which are necessary in order to obtain satisfactory results with it. The instrument is beautifully executed by Mr. Grubb, who himself contrived the subordinate mechanism, by which the requisite movements are effected with perfect ease to the observer.

---

April 23d, 1838

SIR WM. R. HAMILTON, A. M., President, in the Chair.

---

Dr. Apjohn communicated a paper upon the subject of a new and very complicated compound, consisting of iodine, iodide of potassium, and the essential oil of cinnamon.

This compound he stated to have been first observed in the winter of 1837, in a solution prescribed by a medical gentleman of this city, of iodine and iodide of potassium in cinnamon water. It is best obtained by adding to a gallon of cinnamon water four ounces of iodide of potassium, and forty grains of iodine, dissolved in a minimum of cold water. Upon admixture, the solution becomes turbid, and if the temperature be at or close to 32°, the deposit becomes crystalline, and slowly subsides. The properties of these crystals were detailed, and a succint account given of the different steps of the process employed for effecting their analysis.

As the result of a number of experiments, the author arrived at the following numbers, expressing the composition of 100 parts of the compound.

| | |
|---|---|
| Iodide of potassium . . | 12.55 |
| Iodine . . . . . . . | 28.14 |
| Oil of cinnamon . . . | 59.31 |
| | 100. |

The empirical formula deducible from these results is

$$IK + I_3 + ci_6$$

in which *ci* represents $C_{18}\ H_8\ O_2$, the atom of oil of cinnamon, as determined by Dumas; and the most probable rational formula he stated to be

$$IK + 3\,(I + ci_2).$$

Calculating from this formula, its composition would be

| | |
|---|---|
| Iodide of potassium . . | 12.26 |
| Iodine . . . . . . . | 28.08 |
| Oil of Cinnamon . . . | 59.66 |
| | 100. |

This compound he considered interesting under many points of view; in consequence of its complexity, the peculiarities of its properties, and its presenting a case of incompatibility which had not been previously suspected. Also, as suggesting means which would probably lead to the production of an entire new series of substances having an analogous composition.

Dr. Apjohn stated, that this compound had been brought under his notice by Mr. Moore, of Anne Street, and that he and Mr. Moore had investigated conjointly its properties, and the best process for obtaining it. Of the specimen exhibited to the Academy, sixty-one grains were obtained from a single gallon of cinnamon water.

---

Professor Lloyd read a paper "on a Declination Instrument, made for the Magnetical Observatory of Dublin." The instrument, which was made by Mr. Jones, of London, was laid upon the table of the Academy.

The principle employed in this apparatus is the well known optical principle of the *collimator* of Kater. The needle is a rectangular bar, twelve inches in length; it is provided with three sliding pieces, one of which is at the middle of the bar, the other two near the ends. To the former is attached the suspension thread; one of the latter carries an achromatic lens, whose focal length is somewhat less than the length of the bar; the other, which is to be adjusted to the focus of the lens, contains a cross of wires. The rays proceeding from the cross are, under these circumstances, refracted parallel to one another, and to the line connecting it with the centre of

the lens,—which may be denominated the *line of collimation* of the instrument. If, then, a telescope be placed so as to receive any portion of this parallel beam, the cross will be seen in the direction of the line just mentioned; and that, independently of the exact adjustment of the telescope.

The frame-work of the apparatus consists of two pillars of copper, firmly attached to a massive slab of marble.—The height of the pillars is eighteen inches; they are connected by two cross pieces of wood, one at the top, and the other about five inches from the bottom. In the centre of the top piece is the suspension apparatus, the plan of which has been adopted from the torsion balance of Coulomb, as described by M. Pouillet. It is provided with a divided circle, for the purpose of determining the amount of torsion of the thread, and of correcting it.

The magnetic bar, suspended by parallel silk fibres, is enclosed in a rectangular wooden box, to preserve it from the agitation of the air. A glass tube, between the two cross pieces, surrounds the suspension thread, and completes the enclosure of the instrument.

The box is entirely distinct from the rest of the apparatus; it is made in two halves, which are joined at the sides by dovetails; so that it may be put on when the needle has been fully adjusted. There is a circular window at each end of the box. That nearest the observer is made of *parallel* glass; and is contained in a frame which has a motion of rotation in its own plane. By a revolution of 180°, the prismatic error, if any, is corrected. The window at the other end of the box is for the purpose of illumination. In order to determine the internal temperature, the box is provided with a small thermometer, the bulb of which is within, and the stem (bent at right angles) on the outside front.

It is intended to employ this apparatus for three purposes; namely, to determine, first, the *magnetic declination*; secondly, the periodical and the irregular *variations of the declinations*; thirdly, the corresponding *variations of the horizontal magnetic force*.

In using the instrument in the determination of the declination, it is to be combined with the theodolite and the transit instrument. The transit instrument is to be fixed close to the southern window of the observatory; there being also an aperture in the roof for the purpose of adjusting it to the meridian by means of the pole star and δ *ursæ minoris*. The centre of the theodolite is placed, as nearly as possible, at the point where the line of collimation of the transit instrument intersects that of the magnetic bar. When an observation

is made, the telescope of the theodolite is directed to the lens of the magnetic collimator, and the vertical wire of the latter is made to bisect the cross in the focus of the telescope. When this is done, the line of collimation of the telescope is parallel to that of the magnetic bar. But as the latter line may not coincide with the *magnetic axis* of the bar, a similar observation is to be made with the bar inverted; and the mean of the two readings will obviously give the direction of the *magnetic meridian*, freed from the error of collimation. To determine the angle between this and the *true meridian*, the transit telescope is to be turned over, and employed as a collimator. The telescope of the theodolite being directed to its object glass, the middle wire in the focus of the transit is to be observed, in the same manner as the wire of the collimator bar was in the former part of the observation. The line of collimation of the theodolite telescope is then in the *true meridian;* and the angle read off on the limb is the supplement of the declination.

In observing the diurnal and irregular variations of the declination, the reference to the meridian is not required. Here, therefore, the theodolite and transit instrument are unnecessary, and the former will be replaced by a *fixed* telescope, furnished with a finely divided micrometer scale in its focus.

A similar apparatus serves for the determination of the changes in the horizontal magnetic force. It is only necessary to modify a little the suspension arrangement, and to substitute for the single thread *two equidistant threads;* as in the torsion electrometer of Mr. Snow Harris. The needle is then to be turned by the force of torsion, into a position at right angles to the magnetic meridian; in which position the momentum of the magnetic force is greatest. The chances of position of the bar (read off as before by a fixed telescope with a micrometer scale) will enable the observer to deduce, by an easy formula, the corresponding changes of the magnetic force. Professor Lloyd then entered into some details connected with the theory of the instrument as thus employed; and he showed in what manner it was to be adjusted, so that a given variation of the magnetic force might produce the greatest variation in the position of the bar.

---

A paper was read by Edward S. Clarke, Esq., on an Improvement which he had lately made in the Sustaining Battery, and on the size proper to be given to the zinc element of sustaining batteries in general.

The author alluded to the decline of voltaic power which occurs during experiment, and ascribed to M. Becquerel the credit of having assigned its true cause; referring it, as this philosopher did, to a transfer of the decomposed substances to the respective plates, in such a way as to produce secondary currents moving in a direction reverse to the primary current.

Mr. Clarke also alluded to the fact, that Becquerel was the first person who, to remedy this evil, adopted, in 1829, the use of a membranous partition, and two different liquids, to separate the respective metals; but added, that the form this philosopher adopted was imperfect, in consequence of the difficulty of affixing the membranous portion staunchly to the sides of the square glass box which contained the two different fluids.

The author, after referring to the sustaining battery of Professor Daniel and to the modification of that apparatus adopted by Mr. Mullins, exhibited to the Academy a battery which he had devised to remedy a defect affecting all previous combinations, and in which each surface of the hollow zinc cylinder had, as first recommeded by Mr. Wollaston, a surface of copper opposed to it. An account was also given of several experiments which showed the advantage of his form in calorific and electro-magnetic experiments.

Mr. Clarke's improvement consists in attaching a ring of zinc by zinc rivets to the top part of the outside of the hollow cylinder of zinc used in the arrangement of Mr. Mullins, and drawing a bladder over this cylinder, to which it is secured by a cord to the ring; and in replacing the earthenware jar by a copper cylinder, which is furnished with a mercury cup, as are also the zinc cylinder and the central copper. The central copper and the outer copper case are connected by a wire dipping into the cups. A solution of sulphate of copper is poured, as well into the outer case of copper, as into the bladder surrounding the central copper, and muriate of ammonia into the bladder enclosing the zinc.

The author concluded by detailing some experiments, tending to show that, (contrary to the opinions of M. Marianini and Mr. Mullins) the maximum effect is obtained when the surface of the zinc element is equal, or nearly so, to that of the copper.*

* We have received Mr. Clarke's original paper, which, with the illustrative figures of the battery, will appear in our next number. Edit.

## XVII. MISCELLANEOUS ARTICLES.

### *Note on Mr. Cooper's Letter to Mr. Harper, respecting Joyce's Stove.*

Having made a very careful perusal of Mr. Cooper's letter to Mr. Harper, on "Joyce's stove," we are prepared to furnish our readers with some additional information respecting the use of that apparatus. We shall premise by observing, that the first impression which the reading of Mr. Cooper's letter left on our mind was simply this: that, sanctioned as it is by Professor Brande, it is much better calculated to be serviceable to the venders of Joyce's stoves than to the purchasers of them; for it is sufficiently obvious, even at first sight, to a scientific man, that Mr. Cooper's experiments are not only deficient in point of number, but appear to us, whether selected or not, to be eminently calculated to convey to the unwary, an exceedingly partial view of the real value of the apparatus.

It might seem necessary to enquire why Mr. Cooper's experiments were made in a natural atmosphere which required no artificial heat to make it agreeable? Are the purchasers of these celebrated stoves to understand that they are to be used only at those times when the natural temperature of the room is 62° Fahr.? Is this the information they receive at the sale-rooms? If, on the contrary, Joyce's stove be intended to be more generally useful, and to compete with other modes of warming apartments, we readily perceive that Mr. Cooper's experiments would have afforded a much more perfect view of the *real* value of the stove; and, consequently, would have been of far more importance to the public, had they been made at those *natural temperatures* of the atmosphere in which artificial heat is generally wanted.

If proper experiments had been made during the severe weather of last winter, and that no other artificial source of heat than that produced by the combustion of three ounces of charcoal per hour, had been in the room, Mr. Cooper knows very well, that the results would have been very different to those stated in his letter.

It is here we find that the investigation is imperfect or partial; and that there is a blank at the very point from which the only really important information is to be derived. But as it is our intention to dispose of that which *has been* done,

before we offer any remarks on that which *ought to be* done: we will now endeavour to apply the data which Mr. Cooper's *summer* experiments have afforded, to the solution of some of those problems which a winter's atmosphere and Joyce's stove might probably produce.

If the charcoal burned at an equable rate, from "eleven o'Clock in the evening" till "ten o'Clock the following morning," the temperature of the air of the room would be at a maximum at the latter period. Hence, from this fact, combined with other data furnished by Mr. Cooper's letter, we are led to understand, that in order to elevate the temperature of 2,000 cubic feet of air from 62° to 72·5°; or through a scale of about 10°, requires 33 "ounces of the *prepared* fuel;" and eleven hours of time. And, according to the other experiment, an increase of 13° of temperature would require "forty-four ounces and a half of fuel," and "fifteen hours in a closed apartment." With many persons *time* is as valuable as *fuel;* and the economizing the former of far more importance than that of the latter.

Let us now assume, for the convenience of illustration, that the conducting faculty of air, and other bodies in the apartment, for heat, is a *constant quantity* at all temperatures. Then, according to Mr. Cooper's experiments, we are led to understand, that, in order to elevate the temperature of 2,000 feet of air, in a close room, from 32° to 72·5°, would require *four* of Joyce's stoves, 132 ounces of fuel, and eleven hours of time; and, at some periods of the severe frost of last winter, no less than *six* such stoves, 198 ounces of fuel, and eleven hours of time would have been necessary to raise the temperature to 72·5° Fahr. Or, it would have required *five* stoves, 165 ounces of fuel, and eleven hours of time to have heated the air of Mr. Cooper's room to the temperature at which he found it previously to the introduction of the *experimental stove.**

Respecting the formation of carbonic acid, the per centage would be proportional to the quantity of the charcoal consumed in the standard period of eleven hours; so that by adhering to Mr. Cooper's data, it appears that by raising the temperature to 72·5° from the freezing point, the air of the room would contain about four per cent. of carbonic acid; and during cold weather, such as we experienced last winter, the air of the experimental room, heated to 72·5° Fahr. by the *new mode*, would be charged with carbonic acid to no less than one seventeenth of the whole mass.

* The number of stoves here mentioned is merely for convenience of calculation; Mr. Cooper's experiments affording no other means.

It will have been observed that, in the above calculations, one per cent. of carbonic acid has been taken for every 33 ounces of fuel consumed, which is something more than that which Mr. Cooper's experiments will allow. According to one experiment, the carbonic acid would be 4·5 per cent.; and according to the other, a little more than 4 per cent. for the combustion of 198 ounces of fuel.

The comparisons which Mr. Cooper has made respecting the carbonic acid formed by the combustion of oil, tallow, spermaceti, stearine, and gas, necessarily stand on the same footing as those already noticed; for if one stove give as much carbonic acid as two table lamps, for an elevation of 10° of temperature, then for an elevation of 40°, or from the freezing point to 72°, the stove or stoves would produce as much carbonic acid as eight such lamps; and during such weather as we have experienced in the present year, the "new mode" of heating would produce as much carbonic acid as twelve such lamps. The candles and gas would, of course, follow the same ratio.

The influence which the names of Messrs. Cooper and Brande may have on the public mind on this important topic, calls forth the judgment of every uncompromising scientific man; and humanity demands that a full and faithful investigation of the extent to which the use of Joyce's stove may, probably, deteriorate the air of rooms, be immediately laid before the public in the most efficient manner, to guard the ignorant from imposition, or the timid from fear.

We entertain no doubt whatever of the accuracy of Mr. Cooper's experiments, as far as they have extended; but they fall sadly short of that degree of importance which would have been attached to them had they been carried on at the temperatures of our usual winter seasons, or under circumstances in which the article in question is most likely to be in use. The few calculations we have made are the mere obvious inferences derivable from the scanty data which have been placed before us; not venturing even a single conjecture relatively to the *probable* consequences which might result from any variation of the experiments. But, we should hold ourselves highly culpable of a serious neglect of duty, as Editor of a scientific journal, were we not to warn our readers of the imperfect views which Mr. Cooper's partial investigation of a topic of such vital public importance is calculated to produce; and we earnestly solicit the attention of Mr. Harper to these candid remarks; and suggest to him the absolute necessity of a complete experimental investigation of the matter, and that it be proceeded with as early as the temperature of the atmosphere will admit.

An investigation such as we should recommend would require at least two rooms, of different sizes. The smaller one not to be less than that in which Mr. Cooper made his experiments: and the other much larger.

The experiments should be made at intervals of 10° of temperature, from 50°, downwards to the freezing point at least; and as much lower as our winter seasons would allow. The standard elevated temperature to be 60°.

The rooms ought to be no farther closed than what is usual whilst warmed by ordinary fires, or stoves in common use.

Thermometers ought to be suspended in various parts of the room, and at different altitudes. The experimenter ought to remain in the room for several hours at a time, at intervals, during the whole period the stove is kept in play. The thermometric observations, and test experiments for carbonic acid, ought to be made as frequently as convenient: and the latter on air taken from various altitudes in the room: and the character of the air at each individual altitude should be distinctly stated.

There might be one stove or more in each room, as found necessary; and the total consumption of fuel in each stove ought to be strictly noted. EDITOR.

---

## *On Solid Carbonic Acid.*

*Letter from M. Thilorier to the Academie de Sciences of Paris.*

"I have the honour of now announcing to the Academy that I have just finished a second memoir upon liquid carbonic acid, in which, after having successfully examined the different parts of this body, its *specific gravity*, which is so variable that, from 32° to 86° Fahr., it successively runs through the whole scale of densities from water to that of ethers; its *dilatability*, which is four times greater than that of air itself; its *pressure* and the *weight* of its vapour; its *capillarity*, and especially its *combressibility*, which is a thousand-fold greater than that of water, I have succeeded in determining in the most exact manner, the uniform and constant law which regulates all these phenomena, which at first view appear altogether independent of each other. The Academy will without doubt learn with interest, that, by means of a very simple apparatus, I have succeeded in producing instantly, and economically, a mass of solid carbonic acid weighing an ounce and an ounce and a quarter, and which the experimental chemist may beneficially employ. My first experiments on cold, which I have already presented to the Academy, were made by directing a stream of liquid

carbonic acid upon the bulb of a thermometer, or on tubes which enclosed the different substances upon which the action of the cold was tried. This method had the serious inconvenience of wasting a great quantity of the liquid, and of leaving some uncertainty upon the maximum of the cold produced. The facility and abundance with which I now obtain the solid carbonic acid has supplied me with a method of experimenting which is infinitely preferable. The bulb of the thermometer having been introduced into the centre of a small mass of solid carbonic acid, at the end of one or two minutes the thermometer became stationary and stood at—194° Fahr. Some drops of ether and of alcohol poured upon the solid mass did not produce any appreciable difference less or more on the temperature. Ether forms a mixture which is half liquid, and of the consistence of melting snow; but alcohol, in combining with solid carbonic acid, congeals, and produces a hard, brilliant, and semi-transparent ice. This freezing of anhydrous alcohol only takes place in the act of mixture; when isolated, as in a silver tube, in the midst of a mass of solid carbonic acid, the alcohol undergoes no change whatever. The mixture of alcohol and solid carbonic acid begins to melt at—185° Fahr, and starting at this point, the temperature does not vary any more. Thus we can obtain from this extreme limit a point as fixed as is supplied by that of melting ice. If after having formed *a small coppel* of solid carbonic acid, we pour into it three or four drachms of mercury, it is seen to congeal in a few seconds, and to remain in this new condition so long as an atom of solid carbonic acid remains, that is to say, for twenty or thirty minutes when the weight of the coppel is from two to three drachms. I have already said that the addition of ether or alcohol did not augment the real degree of the cold, but, by giving the solid carbonic acid the power of moistening bodies, and of adhering more intimately to their surface, these substances much increase the frigerating effects. A piece of solid carbonic acid, on which some drops of ether or alcohol are poured, becomes capable of congealing fifteen or twenty times its weight of mercury. The promptitude with which it is converted into the solid state, the mass in which it is effected, and which may easily exceed half a pound, and its continuance in this new condition, which may be maintained as long as you like, with the single precaution of placing the metallic mass upon a base of solid carbonic acid, leads me to believe that this method of freezing mercury will henceforward be substituted for all those which have been previously employed.

*Process for Ink devoid of free acid. By* R. Hare, M.D.

Writing Ink is usually constituted of the tanno-gallate of iron and a portion of sulphuric acid which had existed in the copperas, or sulphate of iron employed as one of its ingredients, the tanno-gallate being suspended and the acid dissolved in the water. This free acid is injurious to iron pens. Dr. Hare has observed that when an infusion of galls is kept over finery cinder till saturated, it forms a beautiful ink, in which of course there is no free acid.

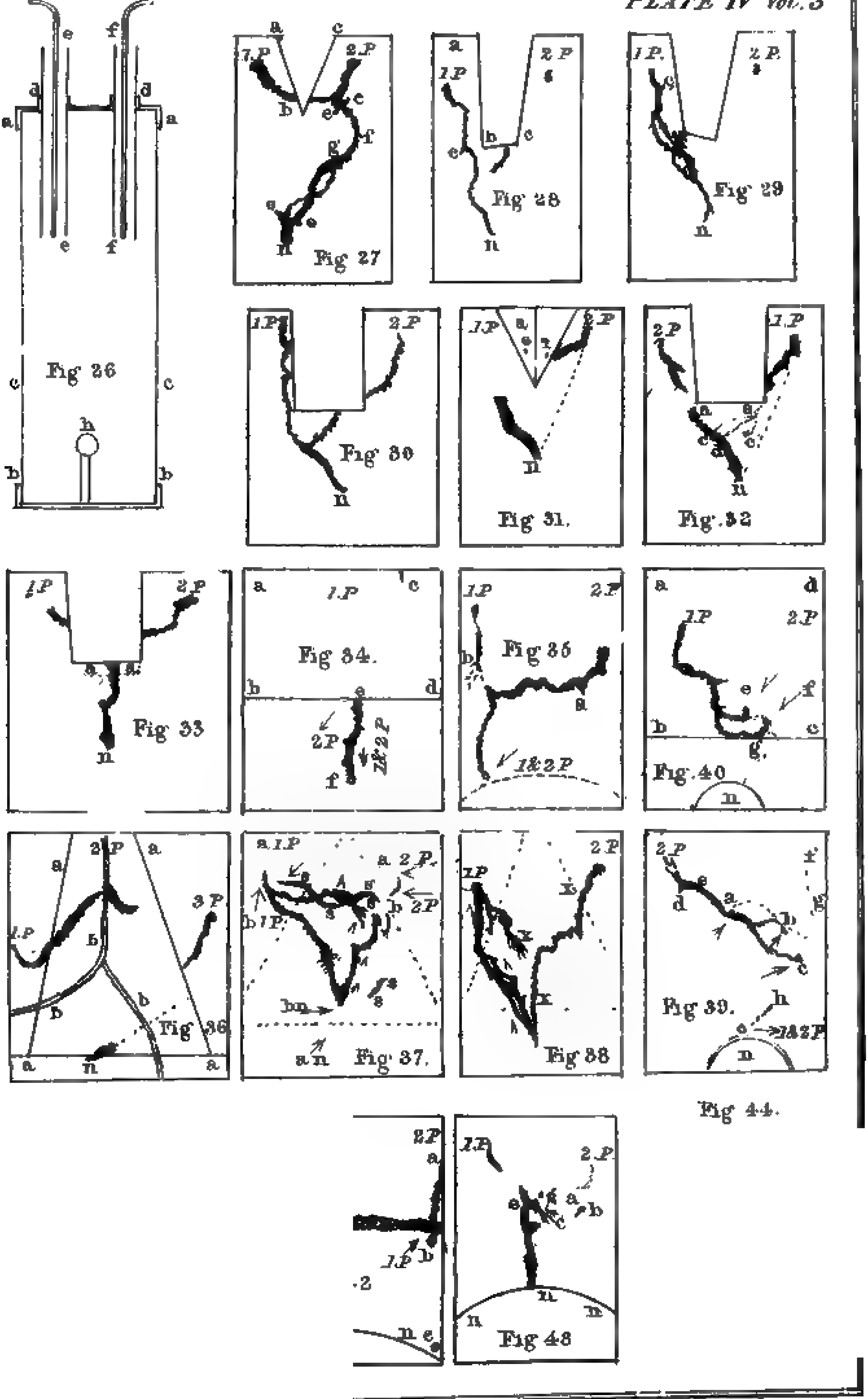
This ink is rather more prone to precipitate than that made with sulphate of iron, and this propensity is not counteracted by the addition of gum arabic. But on the other hand, it has the advantage of being easily suspended again by agitation, not forming any concrete matter insusceptible, like tommon ink grounds, of that distribution in water, which is necessary to good ink. The tanno-gallate of iron when obtained from a filtered infusion of galls and finery cinder, as above described, on being evaporated to the consistency of thick molasses, gum arabic in due proportion having been previously added, forms a pigment which might, it is conceived, supersede India Ink. When completely dried, it glistens like jet.

This tanno-gallate of iron only requires to be dried and ignited at a low red heat, in order to be converted into a pyrophorus. A few years ago, Dr. Hare ascertained that, by a similar ignition in close vessels, cyanoferrite of iron, the Prussian blue of commerce, gave a pyrophorus. But as the pure cyano-ferrite of iron, resulting from the addition of the ferro-prussiate of potash, more properly the cyano-ferrite of potassium, to a ferruginous solution did not form a pyrophorous; he was led to believe the presence of sulphate of alumine in the commercial Prussian blue was the source of the difference, probably by being converted into a sulphide of aluminium, or potassium.

The production of a pyrophorus from the tanno-gallate proves that iron and carbon, when in a state of minute division, are capable, by ignition in close vessels, of acquiring that property of spontaneous combustibility which entitles the body which possesses it, to be called a pyrophorus.

In truth these results are consistent with some facts mentioned by Berzelius, as having been ascertained by Mitcherlic, respecting the spontaneous combustibility of iron, reduced from the state of magnetic oxide to that of the pure metal in an extreme state of division. Also the spontaneous combustibilityof the residue resulting from the ignition of the oxide of iron at a red heat.

ANNALS OF ELECTRICITY &c. PLATE IV Vol. 3



THE ANNALS

OF

*ELECTRICITY, MAGNETISM,*

*AND CHEMISTRY;*

AND

**Guardian of Experimental Science.**

*AUGUST*, 1838.

XVIII. *On an improved form of the Voltaic Sustaining Battery, and on the size proper to be given to the zinc element of sustaining batteries in general.* *By* EDWARD S. CLARKE, Esq. M. R. I. A. *&c. &c.*

Read April 23d, 1838. See Page 77.

Scarcely had the voltaic battery, in all its varying forms, attracted the attention of philosophers, when it was observed that the powers of this instrument sunk very rapidly indeed during the progress of any experiment, continued even for a comparatively short time.

This deterioration of power which was so rapid in the closed circuit, was observed to take place but very slowly when the poles of the voltaic arrangement were not united; and it was noticed that the primitive power was very nearly restored (when the exciting liquor was not too active, or the experiment too long continued) by opening the circle, and allowing the apparatus to remain inactive for a short time; but in proportion as the chemical powers of this instrument became unfolded, and the necessity for long continued currents (which would not fall below a given intensity) observed, it became important to remove, if possible, this source of error; and with this view two methods were devised and practised about the same time, both by Professor Marianini, at Venice,* and by Sir Humphrey Davy, in England.† The first method consisted in having spare troughs, and replacing the acting trough by one of them as soon as its intensity was observed to have fallen to the given limit; and it will be obvious, that if a vol-

*Annales de Chimie, Tome XXXVIII. page 337.

† Transactions of the Royal Society of London, year 1826.

taic arrangement recovered its power by repose, in the same time as it lost it during action, then only one spare trough would be required: but this was found not to be the case, and hence so many spare troughs, and plus one, were required as the time necessary to restore the primitive intensity by repose, exceeded that in which the deterioration took place. The second mode consisted in having two similar arrangements, each intended in turn to transmit the current; the other which had lost its intensity being in the interim subjected, for a few seconds, to a current from a much larger battery, sent through it in a direction reverse to that of its own natural current: yet both of these methods were troublesome and expensive. And although it appears from the methods which both Sir Humphrey Davy and M. Marianini adopted, in order to restore the lost intensity, that these philosophers were at that time acquainted with the cause of the decline of galvanic power. Yet, so far as I have heard, it was not clearly pointed out until M. Becquerel, in a paper read to the French Academy, in the month of February of the year 1829,* demonstrated it to consist in the transfer of the decomposed substances to the respective plates, in such a way as to produce currents moving in a direction reverse to the principal current; and this philosopher at the same time described experiments in which he used (with greatly increased effect) in each voltaic arrangement, two different fluids, namely, sulphate of zinc for the zinc plate, and nitrate of copper for the copper plate; these fluids being separated from each other by partitions of thin animal membrane, ("baudruche,") inserted into the sides of a box of glass, and in this way M. Becquerel often obtained an electric current, of which the intensity remained constant and uniform during an entire hour. Yet he describes the operation as troublesome, in consequence of the difficulty attendant upon affixing the membrane staunchly to the sides of the box.

In the year 1836, Professor Daniel demonstrated the same fact still more clearly, in a paper read to the Royal Society; and also described a voltaic arrangement of a circular form, which consequently enabled him to use a circular membrane, which being naturally of the required shape, at once removed the mechanical difficulty under which M. Becquerel had laboured. In Mr. Daniel's form, a small solid cylinder of zinc, together with its charge of diluted sulphuric acid, is inserted into a portion of an ox gullet, and a saturated solution of sulphate of copper is poured into the outer copper, which

* Annales de Chimie et de Physique, Tome XLI. page 21.

supports the membrane in its centre. This membrane thus placed between the zinc and copper elements being perfectly staunch, prevented any considerable* mixture of the different liquids, but more especially preventing the transfer of positive particles to the negative metal; which transfer M. Becquerel first, and afterwards more fully, Professor Daniel himself, had shown to be the cause of decline of power in all former voltaic arrangements; and certainly the form thus introduced by Professor Daniel was the most efficient which had been given to the voltaic arrangement (so far as chemical decomposition was concerned) from the period of its discovery by the illustrious Italian to the time of the Professor's improvements.

It is, however, true that the form which this philosopher adopted for the zinc part of his battery (namely, a solid cylinder), could not be enlarged to any considerable extent without too great an increase of weight; and hence this battery appears not well adapted for experiments requiring a large *quantity* of the electric fluid.

The form subsequently devised by Mr. Mullins, in which the membrane was placed on the copper, and a *hollow* cylinder of zinc, of large surface, was used to surround it, possessed at once all the useful properties of the constant battery of Professor Daniel, with the additional advantage of considerable surface without that great increase of weight, which in the Professor's form, would have been necessary to effect the same purpose; and this form which Mr. Mullins has given the name of the "sustaining battery," is certainly very advantageous in all those experiments which require considerable quantity coupled with constant intensity. It has, however, been a subject of regret, that whilst in Mullins's battery both surfaces of the hollow cylinder of zinc suffer corrosion,† that

* I say "any *considerable mixture*," because that in time a certain small mixture of the solution of the cupreous sulphate with that of the ammoniacal muriate does take place, although it does not appear to injure the action much. Perhaps this mixture may arise from the action by endosmode, particularly as M. Dutrochet has rendered very probable that electricity is the cause of all endosmode actions.—Vide Annals de Chimie, Tome XXXVII. Indeed this action of endosmode appears useful to the existence of the constant current, in consequence of the sulphuric acid liberated from the decomposed sulphate of copper permeating the bladder and acting on the zinc cylinder.

† It is clear that Dr. Page's proposal, in the American Journal, to place the bladder on the zinc cylinder, and to have the copper outside of this as a jar, instead of inside as in Mr. Mullins's form, does not at all interfere with my priority as the introducer of the

only one of them was effective in the production of the voltaic currents, the other not having any copper surface opposed to it. It was to remedy this evil that I adopted the form shown in section in fig. 44, Plate V., where *a* is the interior moveable cylinder of copper covered with a bladder, as in Mullins's arrangement; *b*, the hollow zinc cylinder, having attached to its top (by zinc rivets) a ring of cast zinc, formed as shown in section at fig. 45. On this ring the bladder is tied, and the space of half-an-inch which exists between its mouth and that of the zinc cylinder to which it is attached, proves useful in charging the bladder which surrounds it. *c* is a copper vessel provided with a bottom, and staunch in all its parts.

In this form of the arrangement, the two copper parts, *a* and *c*, are to be connected together by a *thick* copper wire, *z*, dipping into the mercury cups, *d* and *e*, as shown in fig. 44. A saturated solution of sulphate of copper is to be put into the bladder surrounding the copper, *a*, as in Mullins's arrangement; a solution of muriate of ammonia composed of one part saturated solution added to five parts water, into the bladder surrounding the zinc cylinder, *b*, and a saturated solution of the cupreous sulphate is to be poured into the exterior copper vessel, *c*; thus the fellow zinc cylinder, enveloped in a bladder charged with muriate of ammonia, has at each of its faces a surface of copper opposed to it; an advantage which the elementary battery of Wollaston has long since taught us duly to appreciate in every case where quantity of electricity is desirable; indeed it is very striking to observe the advantage in the present case, when the wire for connecting the two cylinders of copper, *a* and *c*, is inserted in the mercury cups, *d* and *e*, tbe arrangement then igniting a length of wire, in which before there was no visible ignition; and the physiological effects* are also very much increased by this act. I think,

form described in this paper: for in Dr. Page's form only one of the zinc surfaces was opposed by copper, whilst the introduction of a form which admits the use of the double copper is the cause of the superiority of my form of battery, and has been the main object of my consideration.

* As when the shock is taken from the secondary current excited in the long wire surrounding an electro-magnet when battery contact is broken. A shock easily taken when only the single interior copper is used, becomes absolutely intolerable when the connecting wire *z* is employed to unite the two copper cylinders *a* and *c*; and if Dr. Page's or Mr. Golding Bird's apparatus be used to break contact in that experiment, the increased force and velocity with which the instrument strikes against the regulating stop or electro-magnet produces an effect upon the auditory organs which impresses

however, that the following synoptical table will exhibit the benefit of this arrangement in a tolerably clear point of view, being the result of some experiments made with the ordinary Mullins's sustaining battery, and with my form of the apparatus. The exciting charge of muriate of ammonia was identically the same in each experiment, as was also the zinc cylinder employed. The mode of operation pursued being to try the sustaining battery of Mullins only provided with a ring on the zinc for securing a bladder on the same when required, but which bladder was not used in the first set of experiments, it was thus the ordinary sustaining battery; the results of the experiments then made were noted, and afterwards a bladder was secured about the zinc cylinder, and it was inserted into the copper vessel *c*, which united to *a* placed within *b*, it formed my new arrangement. Here as the same zinc and the *same* charge was employed in each set of experiments, the result may be considered as truly representing the relative electric power of the two arrangements.

*Synoptic Table, No.* 1.

| *Sustaining Battery, Mullins's Form.* | *Sustaining Battery as improved by me.* |
|---|---|
| Ignited nearly half an inch in length of platina wire about 1·100th of an inch diameter. | Ignited about $\frac{3}{4}$ of an inch in length of same wire, when it formed one straight line between the polar copper wires; but when the platina wire was folded as in fig. 46, so as to form two media for transmission of the large quantity of electricity furnished by the double coppers, it ignited two portions *f* and *g*, each 5-8ths of an inch in length and 1-90th of an inch diameter. |

the experimenter with a very strong conviction of the superior power of my form of battery. When also the voltaic current is intentionally made so feeble as to be incapable of moving the above apparatus when only one copper is employed, motion immediately commences as soon as the two copper cylinders, *a* and *c*, are connected by the uniting wire *z*. It is necessary to observe that all connecting or discharging wires used in this arrangent must be much thicker than those usually employed in other batteries of equal size.

| | |
|---|---|
| Permanent deflection of magnetic needle 7½ inches long and 80 grains in weight, and having a directive power, such that it made 12 vibrations per minute, 56 degrees | Permanent deflection with same needle, 70 degrees. |
| An electro-magnet of ¾ inch diameter with limbs 4½ inches long, surrounded by four super-imposed coils of copper wire 1-14th of an inch thick, supported 40 lbs. | Same electro-magnet supported 60 lbs. |

In using the above magnetic needle, a thick wire was employed to form a complete convolution. Thus a very obvious advantage attends the use of the sustaining battery in the improved form which I have given to it.

It is necessary to state that the zinc element of the sustaining arrangements (being the same in each set of experiments) was nearly the full height of the copper; previous experiment with zinc elements of different sizes (still using the same copper) having shown me that when the connecting wire is sufficiently large to transmit the full current, a greater quantity of electricity is developed by sustaining batteries furnished with zinc nearly equal in size to that of the copper, than by those of which the zinc element is of much smaller surface; and this induces me to describe the experiments which have impressed this conviction. I am aware that the experiments of Professor Marianini, (of the Venice Lyceum,) made in 1825, led him to the conclusion that a voltaic arrangement, whose zinc element presents a surface of from 1-10th to 1-5th part of the copper, acts as efficiently as one of which the surface of the zinc equalled that of the copper. And in 1836, Mr. Mullins recommended the use of smaller surfaces of zinc than of copper; but this gentleman advised that the zinc should present a surface of from 1-4th to 1-3d part of that of the copper, instead of from 1-10th to 1-5th part as in the practice of the Venetian Professor. Yet the following experiments which I made in August last to ascertain whether zinc, even of the proportion recommended by Mr. Mullins, would answer the purpose as efficiently as one equal in surface to that of the copper, appear to me conclusively to prove that zinc, even of the size Mr. Mullins has recommended, is inferior in voltaic action to that possessing surface equal to the copper of the same arrangement.

* Annales de Chimie, Tome XXXIII.

The copper cylinder employed in these experiments being a part of one of Mullins's sustaining batteries, was 7 inches high and 3¼ inches diameter, and I furnished it with two zinc elements, each 4¼ inches diameter; but one of them was only 2½ inches high, the other being 6¼ inches: these two cylinders, which were both new, were used in turn with the same copper and the same charge, in order to obtain comparative results. The copper and zinc cylinders were each furnished with a thick copper connecting wire, instead of the narrow copper ribbon sold with this instrument; want of attention to which particular having, in my mind, occasioned the erroneous impression, that as much electricity was produced with a small zinc cylinder as with the larger one; whereas this was not really the case, but the small ribbon was probably unable to transmit for experiment any larger quantity than the small zinc afforded.

The exciting charge employed consisted of one part saturated solution of muriate of ammonia added to five of river water, both by measure, and the conducting liquor, next the copper, was a saturated solution of sulphate of copper.

RESULTS.

With large zinc 6¼ inches high, and 4¼ inches diameter, the current being transmitted along a thick copper wire making one convolution around a light magnetic needle 7½ inches long, resting by its agate cap upon a steel point, caused a permanent deflection of 50 degrees.

The small zinc 2½ inches high and 4¼ inches diameter, with same copper and same charge, only changed by removing a part in order to have it no higher than the top of the small zinc, caused a permanent deflection of 42 degrees.

Small zinc ignited scarcely 3-8ths of an inch in length of platina wire, 1-100th of an inch in diameter, when used in a straight single line, between the thick polar wires of copper; but when this platina wire was folded around these polar wires in such a way as to form two media for the transmission of the current, as shown in fig. 46, where *f* and *g* represent the platina, and C and D the section of the polar wires, no visible ignition was produced. The quantity of electricity furnished by this small zinc being unable to produce ignition when thus divided into two channels.

The same copper part of the battery and same charge, as at first, being used with the larger cylinder of zinc, 6 inches in height, half an inch of same wire was ignited when applied singly, and when folded so as to afford two channels for the current as in fig. 46, two portions *f* and *g* were ignited, each

being 3-8ths of an inch in length; thus clearly showing the very much larger quantity of electricity furnished by the larger zinc.

An electro-magnet of $\frac{3}{4}$ inch diameter, having limbs $4\frac{1}{2}$ inches long, surrounded by four separate layers of insulated copper wire (about 1-14th of an inch thick) placed over each other, and each continued from one end* of the bar to the other, sustained with the small zinc 27 pounds, with the large zinc 50 pounds.

Thus, all these experiments, but particularly those of ignition of wire and induction of magnetism in soft iron, show that it is better to use in sustaining batteries, zinc of a size nearly equal to the copper immersed than either of the sizes recommended by Professor Marianini or by Mr. Mullins.

I may here add that two batteries of the improved form I have described, of which the zinc element of one was 6 inches in height by 4 inches in diameter, and of the other $6\frac{1}{2}$ inches in height by $5\frac{1}{2}$ inches in diameter, (when used with the four copper cylinders connected together, and the two zinc cylinders also connected between themselves) ignited 4 inches of platina wire about 1-64th of an inch in diameter, a result which, viewed with reference to the individual igniting power of each pair, seems an astonishing increase of effect, and what appears still more strange is that the brilliancy of the igniting effect is greatly increased by connecting the arrangements consecutively, instead of in the form just described; a mode which I believe has always hitherto been regarded as the best suited for the production of calorific effects. Perhaps this may arise from some peculiarity of sustaining batteries. I did not, however, repeat the experiment very often, and therefore cannot be certain that this phenomenon will always occur.

Mr. Lynd, Optician, 26, Capel Street, made the double copper battery, under my instructions.

---

## XIX. *Remarks on the construction of Magnetic Observatories and the instruments which they should contain.*

From the German of Professors Gauss and Weber.

The instruments by means of which the observations here detailed were made, differ materially in many respects from those which have hitherto been constructed for similar purposes; and in order to form a correct notion as to the value to

* The wire was not rolled at all closely over the bended part.

be attached to the results which they give, it is consequently necessary to enter fully into the details of their construction.

What has been already laid before the public in former papers and notices (see, for instance, the work entitled "Intensitas vis magneticæ terrestris ad mensuram absolutam revocata, auctore. F. C. Gauss, Gottingæ, 1833." The Gottingen Gelehrte Anzeigen, 1832, p. 2041; 1835, p. 345; and Schumacher's Jahrbuch, 1836, p. 1.) might indeed be held to be sufficient; yet the detailed description of the apparatus employed as given below, especially when illustrated by the accurate figures with which it is accompanied, will not only facilitate our knowledge of the instruments used, but also enable every competent artist to construct similar ones for himself.

For the figures here given, those instruments only have been selected, which are made by Meyerstein, of Gottingen, and of which he has forwarded complete sets to Bonn, Dublin, Freiberg, Greenwich, Kasan, Milan, Munich, Naples, and Upsal; and which are nearly identical with those in Gottingen, made by Apel; and those in Cracow, Leipzig, and Marburg, by Breithaupt, of Cassel.

No description is here given of those smaller instruments which have been used in some places, inasmuch as experience has shown that they are only to be had recourse to when local circumstances preclude the employment of those of a superior construction.

The most suitable place for the establishment of a magnetic observatory is an oblong room (1)* of about thirty-six feet long in the direction of the magnetic meridian, parallel to which, however, it is by no means indispensable the side-walls should run. In Gottingen, for instance, the direction of the room coincides with that of the astronomical meridian, which at present makes an angle of about eighteen and a half degrees with the magnetic meridian.

The room must be well lighted, especially from the east and west, and also at the end where the theodolite or the telescope and scale are fixed for observing. It is also of importance that the room should be as free as possible from currents of air; for which purpose a double door will always be necessary, and indeed the use of double windows in certain situations will be advisable. The pedestals on which the theodolite (2) and the clock (3) are set, must likewise be perfectly steady.

* The numbers refer to the descriptions given below of the various parts of the magnetic observatory and magnetic instruments.

It is besides desirable that from the spot where the theodolite stands, there may be seen through one of the windows some distant object whose azimuth is known or can be accurately determined.

The floor in the vicinity of the instrument, which stands nearly in the centre of the room, must not contain any iron; and no objects in which iron is used should be placed near it. It is indeed desirable that no iron whatever should be made use of in the room, either walls or roof; but it is noways necessary to carry this precaution so far as to exclude steel pinions or axes from the clock or theodolite which are at five or six yards distance from the middle of the room.

The influence exercised by these masses of steel, supposing them to be magnetic, may be approximately estimated; and the result is that at such considerable distances the effect is too minute to be appreciable. Beyond the limits of the room any small masses of iron have even less influence. Should there, however, happen to be great quantities of this metal, especially in the form of long bars, as, for instance, railings, in the immediate neighbourhood, their action must not be altogether overlooked, although it will be very inconsiderable. If the bars are at the distance of above one hundred feet from the observatory, they may be altogether disregarded, more especially if they are fixtures.

Such a situation is suitable for ascertaining the declination and intensity, and answers also for observations on the variation. Even the inclinations may be taken in the same place, though not without interrupting the other observations. Hence it appears obvious, that where circumstances will permit, a peculiar situation, or distinct apartment, ought to be devoted to the measurement of the intensities, though this room may be at no great distance from that in which the other observations are made. Where, however, no very great nicety is required, the whole of the observations may be made in the same hall; and even small pieces of iron either in or out of the walls, provided they remain stationary, would be of no detriment.

Plate VI, represents the interior of the magnetic observatory of Gottingen, and a ground-plan of it is given in Plate VII.

In order to set the instruments up in such a room as is here represented, it is requisite, in the first place, that a line should be drawn (on the floor, for instance) coinciding with the magnetic meridian.

This line should pass nearly through the centre of the room and continued either to its northern or southern extremity: it must terminate at the spot where the ground is

made perfectly steady for the reception of the theodolite and the clock.

When this foundation has been duly laid and the theodolite placed upon it, the next step is to attach a scale (5) to the stand below the telescope, in such a manner that a plumb-line suspended from the object glass may hang in front of the scale without coming in contact with it. This scale is to be placed in a horizontal position, and at right angles to the magnetic meridian, and must be so constructed as to be capable of being somewhat raised or lowered as occasion requires; and it must be bisected by the magnetic meridian which coincides with the optical axis of the telescope.

A perpendicular is now to be let fall from the ceiling to the floor, in such a manner that the plane of the magnetic meridian may coincide with it, and also with the telescope as above stated. The magnetometer (4) is to be suspended to this line; and the distance of the reflecting surface of the magnetometer (see mirror and mirror frame, 7) from the scale and the telescope, taken together must be exactly equal to the distance of the telescope from a point on the opposite wall, serving as a sight, and to which the telescope may be directed.

At that spot on the ceiling whence this perpendicular has been let fall, the support (8) of the magnetometer, together with its screw lever and thread, (8) must be fastened; and to the thread thus fixed to the screw a weight should now be hung, so as to form a temporary plumb-line; and the position of the support on the ceiling is to be altered continually till the perpendicular coincides with it, and till the support becomes in the direction of its length, parallel either to the north or south wall of the room. When this is done, the height of the support, the telescope, and the scale above the floor are to be measured.

From the height of the first half, the sum of the two last is to be subtracted, and a line is to be formed of parallel threads of raw silk, equal in length to this difference, and strong enough to bear a killogramme above the weight of the magnetometer.

The upper end of this thread is to be fastened to the screw, and the lower end to the cradle (9), in which the magnetic bar may be inserted.

Beneath this bar is placed a large case (10), on the bottom of which are two frames, on to which the bar would fall without risk of injury to the mirror, which is attached to one end of it, should the thread happen to give way.

These preliminary steps having been taken, the nicer adjustments may be proceeded with, that is to say—

1st. Bringing the magnetic axis of the bar horizontal, and fixing the mirror perpendicular to it; or, in lieu of this, the minute angle the axis of the mirror makes with that of the bar may be measured.

2d. Reducing the torsion of the thread to zero at the mean position of the magnet, or measuring the small remaining amount of torsion to be allowed for. *See below torsion bar* 11.

3d. Estimating the amount of the torsion of the thread, and of the magnetic impulse of the bar, at a single deviation. *See below cradle and torsion, circle* 9.

4th. Measuring off upon the wall facing the end of the telescope, the spot for the sight.

The instrument is now ready for measurements of delineation, which consist of—

1st. Ascertaining the azimuth of the sight.

2d. Determining the value of the divisions of the scale.

3d. Observing the vibrations and elongations of the bar. (*See below Controlling Bar* 13).

In the sequel will be given more ample details respecting the precautions to be taken when observing, and which are thus merely noticed at this place.

For measuring the intensity, it is necessary to apply certain measuring rods (10), by which the position of the declination bar is determined. These rules are to be applied horizontally on each side of the case containing the magnetometer, and parallel to the magnetic meridian; so that lines which would connect corresponding parts of their scales should be horizontal, and at right angles to the magnetic meridian. These rules should be fixed at such a height that the declination bar, when laid upon them, may be on a level with the oscillating bar. Should this precaution be disregarded, the perpendicular distance between the declination bar, when lying on these rules, and the oscillating bar, is to be noted.

These rules should be from 15 feet 4 inches to 18 feet 6 inches in length, and they should project about equal distances north and south of the magnetometer. When the room is wide enough to admit of it, it is advisable to connect a third rule with the others, horizontally, at right angles to them. This rod may be so placed below the case of the magnetometer as to be directly under the perpendicular let fall from the central point, between the point of suspension and the centre of gravity of the oscillating bar. Provision must be made

for shifting these rules in the direction of their axes, in order so to fix them that the declination bar, when laid on the suitable points, both in front of and behind the magnetometer, may induce similar declinations.

These arrangements having been completed, the instrument is ready for measuring the intensity.

The first step is to determine the moment of inertia of the declination bar. *See below weights and weight-holder*, 12.

2d. To observe the time required for an oscillation of this bar.

3d. To estimate the declination of a suspended auxiliary bar by the influence of the declination bar, the latter being placed in two positions, either to the north and south, or east and west of the magnetometer.

It may be well to subjoin to this general account of a magnetic observatory, and of the arrangement of its interior, the following details respecting the construction of certain portions of the apparatus employed therein.

*Remarks upon certain parts of the magnetic observatory, and the magnetic instruments it contains.*

1. *The Room.* Plate VI. gives a view of the interior of the room, and Plate VII. contains a ground plan of it. In the former of these plates the south wall is removed, and in the foreground to the right are shown: (*a*) the firm spot for the reception of the theodolite; (*b*) the theodolite stand; (*c*) the theodolite; (*d*) the scale attached to the stand; (*e*) the plumb line bisecting the object glass; near these the clock (*f*) is placed. A line drawn from the telescope of the theodolite to the spot marked by an arrow on the opposite wall, would indicate the direction of the magnetic meridian.

On the ceiling near the centre of the room is fixed the holder for the magnetometer. From this hangs the thread bearing the cradle in which the magnetic bar lies, and on to the front end of which the mirror is fastened in a vertical position. The distance of the telescope from the sight is equal to the sum of the distances from the mirror to the telescope, and to the centre of the scale in front of which the plumb line, as described above, hangs.

2. *The theodolite.* To observe the variations in declination, a simple telescope with a vertical motion which admits of its being from time to time directed to the sight in lieu of the mirror, will suffice; for we thus can see whether the position of the instrument has changed or not. But for measuring the absolute declination a theodolite must be had

recourse to. And as it is necessary not only to read off each division of the millimeter scale, but to estimate parts of division when the mirror is at the distance of about 16½ feet from the instrument, the latter should bear a power of 30 at the least.

2. *The clock.* The time being carefully noted at all observations, a clock beating seconds distinctly must be placed near the observer, with the face turned towards him, so that he may at any instant note the position of the hands, and then count the seconds on from thence. A chronometer may also be employed for this purpose.

4. *The magnetometer.* Independent of a clock and theodolite, which are both assumed to be at hand whenever accurate magnetic observations are to be made, the magnetometer consists of the following parts and which are indispensable for observations in declination; the magnetic bar, the cradle with its torsion circle, the holder with its screw and thread, the mirror with its frame, the torsion bar, the scale, and the damper. To these the following additions must be made for observations in intensity; measuring rods, declination bar, weights, and weight holders.

The magnetic bar as it is connected with the cradle and torsion circle (this latter being in its turn fastened to the holder by the thread), and having the mirror and frame attached to the end of it, is shown in figs. 3 and 5, Plate VIII.

5. *The scale.* Fig. 10, Plate VIII. gives a specimen of the scale which has been employed up to this time, and which must be at least three feet 4 inches in length. Mr. Rittmuller of Gottingen, has lithographed this scale at his establishment, and has had copies of it thrown of on white card-board.

6. *The plumb line in front of the object glass.* A fine dark coloured wire, having at its lower end a weight, is so fastened to the upper border of the ring in which the object glass is set, that it hangs down exactly across its centre.

The little notches of this ring may be used to hold the wire steady; or a hoop with two slits diametrically opposite each other may be made to slip on to the brass collar at the end of the telescope. The wire is to be attached from the upper slit and the hoop is to be so placed that it may hang free through the bottom slit.

On looking through the telescope, at the image of the scale reflected in the mirror, one observes, at the same time, the image of the wire projected on the white scale, and that spot in which it lies in the vertical plane of the axis of the telescope is thereby ascertained.

The point on the floor, which the plumb line on being produced would cut, is to be accurately marked, as it serves to determine the deviation of the theodolite stand from its normal position.

7. *Mirror and mirror holder.* The mirror used for the magnetometer must be most accurately worked, otherwise the image on being magnified 30 times becomes indistinct.

The plane mirrors from the Optical Institute of Utzschneider and Fraunhofer at Munich are the best that have yet been made.

It is advisable that the breadth of the mirror should somewhat exceed its height, because during the oscillations of the magnetic bar the right and left sides of the mirror present themselves in front of the telescope. The most suitable dimensions for the mirror are as follow: from 2 to $2\frac{3}{4}$ inches high, and $2\frac{3}{4}$ to 4 inches wide.

In measuring the distance of the mirror from the scale and from the sight, allowance must be made for the refraction of the rays of light at the front surface of the mirror. It follows in glass from the known constant ratio of sines that that plane is to be considered as reflecting which lies half as far from the back as from the front surface of the mirror. The mirror is to be fastened on to that end of the magnetic bar which is turned towards the telescope, and is to form so complete a system with it that no changes of position, either in one or the other, are to be feared while the observations are carrying on; even if the magnetic bar should be lifted out of its place, and put back in an inverted position. The mirror must moreover be so situated with respect to the bar that its perpendicular may be, if not truly, very nearly, parallel to the magnetic axis of the bar.

The frame as represented at fig. 4, Plate VIII. answers both these ends. It fastens on to the bar by screws, and may be brought into the position required by means of two screw movements at right angles to each other.

8. *The holder with its raising screw and thread.* Many advantages are gained by suspending the thread to which the magnetic bar is fastened from the ceiling, inasmuch as the bar is thereby duly insulated and freed from the jarring to which it is liable from the steps of those walking about in the room; the principal advantage, however, gained thereby, is having a thread of a proper length. If in lieu of a wire (whose elasticity is nearly ten times as great as that of a silk thread of equal strength) we employ a line composed of parallel fibres of raw silk, for suspending the magnetic bar, we shall find that at first it gives considerably, and, hence,

from time to time, it is necessary to take it up so that the magnetic bar with its mirror attached to it may resume their original position. But in thus winding up the thread it is requisite that it should still remain in the same plane in which it hung at first; and this is attained by having a screw in the groove of which the line is fixed, and in which it may be wound up, another equal portion of the screw farther on towards its point turning in a fixed grooved shoulder. The groove in which the line lies thus keeps placing itself anew (upon the screw advancing bodily) in the position it at first occupied, and the line thus hangs continually in the plane in which it hung at the outset. The female screw with the frame in which it is set (and through which latter the worm of the screw projects on the line being taken up) are let into a wooden slide that is attached by means of a nut and spring to a broader board fastened to the ceiling, and in this board the slide may be moved up and down parallel to the north and south sides of the room.

Should the direction of the magnetic meridian be materially changed in course of time, this slide will allow of the magnetometer being kept in the meridian of the telescope.

After thus sliding the holder on the ceiling, an adjustment but seldom required, a fresh sight must be marked on the opposite wall on which the telescope may be directed while in meridian. The line used in suspending the magnetic bar consists of 200 parallel threads of raw silk, each of which is capable of bearing a weight of 463 grains, without giving way The weight to which the line is usually subjected amounts to nearly 4.41 lbs. to which must be added a further load of two weights of 1.10 lbs. each, employed when ascertaining the moment of inertia of the magnetic bar as required for measurements of intensity. The line, therefore, never has to bear above half the weight it will bear without breaking. It may be added that it is about 6.5 feet long, and its torsion is for minute declinations to be estimated at about 1-1000th of the magnetic energy of the bar.

The construction of the line is as follows. The single thread is passed twenty-five times round two glass tubes, whose distance is four times the intended length of the line. This being done, the ends of the thread are firmly knotted together, and the twenty-five-fold ring thus formed is to be stretched tight by drawing the glass tubes apart. A hook with a weight fastened to it is now to be hung on to the line equally distant from the tubes, which latter are to be raised up and made to meet together, and then the two loops formed around them are to be united into one. There is thus obtained a hundred-

fold thread, having a loop both above and below; and on folding this thread again together in a similar manner, we get the line from which the magnetic bar is suspended.

9. *The cradle with its torsion circle.* The amount of the torsion of the line by which the magnetic bar is suspended, is not to be overlooked, when measurement of the absolute declination and intensity are required, even although the line is very thin and of considerable length. In order to measure the amount of this force and by this means to remove its influence by causing the thread to place itself, of its own accord, in such a position that at the mean direction of the bar its torsion may be reduced to zero, it was requisite that one of the ends of the line should be capable of being so turned on its axis, that the angle of torsion may be simultaneously measured.

In order to have convenient access to this adjustment, it is adapted to the lower end of the thread; and, to hinder this motion from being communicated to the magnetic bar, the cradle is made in two pieces (a circle and vernier as it were) which are only capable of motion upon a common vertical axis. This vernier supports the magnetic bar, and is itself supported by the circle. The circle is provided with a peg projecting above the nonius, and has two studs on its upper end, into which the double pointed hook fastened to the lower end of the line catches from below.

This being the construction of the cradle, care must be taken that the vernier in which the magnetic bar lies should rest on the rim of the circle supported by the line, otherwise the friction, should it occur only close to the axis of rotation, brings about a mutual change of position in the two parts owing to the impulse imparted to them by the oscillation of the bar.

The cradle is, moreover, so contrived that the magnetic bar may fit into it either when laid flat or placed edgeways.

This is necessary in order to determine with accuracy by observations in declination at the various positions thus given to the bar in the cradle, how the mirror is situated with respect to the axis of the bar.

10. *The case and the measuring rods.* The case which is used to protect the magnetometer from the influence of currents of air is wide, and admits of easy access to its interior. It forms a cylinder of about 31.5 inches in diameter, and 11.8 in height. It is made of a cylindrical form for this reason, namely, that on measuring the intensity for ascertaining the inertia, a wooden rod of 27.5 inches in length is placed at right angles on the magnetic bar, the length of which is 23.62

inches, and this rod having weights appended to it must have room to oscillate freely in the case when thus connected with the bar.

To perform this part of the experiments without trouble, it is likewise necessary that the lid of the case may be altogether removed, but made to shut down again very close, with merely an opening for the line to pass through. There must also be an aperture for the mirror in the side of the case.

The latter, when the apparatus is not in use, may be closed with a little wooden slide, thus avoiding draughts. Two semicircular lids cover up the top of the case on to which they fit down accurately, and one of them has a small hole in it for the line to hang through. This hole does not lie in the centre of the circle thus formed by the two covers, but it is so contrived that while the line hangs down freely through it, the mirror affixed to the extremity of the magnetic bar may oscillate close against the hole in the side of the case.

This is indispensable, otherwise a small aperture will not suffice for receiving light from the scale upon the mirror and reflecting it from thence to the telescope.

Round about the case, the measuring rods are placed, and it is upon them that another magnetic bar is laid at distances and in positions previously determined, north and south, or east and west, with the intention of causing the oscillating bar to deviate from the magnetic meridian.

11. *The torsion bar and the deviation bar.* To ascertain that the line to which the magnetic bar is suspended, hangs in its natural position (that is to say without torsion) when the bar is in its mean direction, a brass bar of equal length and breadth must be taken, having only a small magnet let into it (in order to shorten partially the duration of the oscillations arising from the elasticity of the line,) and this bar must be laid in the cradle in place of the former one, its magnetic axis having likewise a similar position. To institute this examination with accuracy, this auxiliary bar must be fitted up with a mirror and mirror frame just as the magnetometer itself is. For measurements in intensity, a second magnetic bar of similar dimensions to the principal one is required, and which latter it may replace in the cradle so as to observe its oscillations and to measure its inertia. This second bar is also available as a deviation bar: and for this purpose it is likewise furnished with a little rectangular wooden box, the exterior of whose sides runs parallel to the magnetic axis.

The observer is thus enabled to hit on its suitable position upon the measuring rods with accuracy and despatch.

12. *The weights and the weight holder.* In measuring the intensity, it is necessary that the deviation bar also should be made to oscillate in order to ascertain its inertia thereby.

To attain this point, a slight wooden rod is laid across the magnetic bar as it oscillates, and two equal weights are hung to it, one on either side of the bar, the distances between these weights being varied from time to time. To mark the places on the wooden rod from which the weights are to be hung, and to indicate with accuracy their respective distances from each other, both the weights, each of which weighs 1.1027 lb. avoirdupois, are fitted up with handles bent across them and which have pins running through them turned downwards. These pins are placed upon the summit of fine points jutting out above the surface of the wood. There are several of these pins thus let into the wood at distances of 1.9685 inch, with the exception of the two centre ones which are 3.9371 inches apart. These distances must be laid down with microscopic accuracy.

13. *The controlling bar.* For completing the observations quickly and with accuracy, it is of importance to have it in our power to control the oscillations of the magnetic bar as we please; that is to say, to let the oscillations, when we are measuring their duration, amount to 2 or 3 degrees at first, or when making observations in variation to keep them as small as possible, never indeed letting them exceed 2 or 3 minutes in arc. This is brought about by means of the controlling bar, with the management of which every observer must make himself familiar. It is a magnetic bar of half the length and width, and a quarter of the weight of the principal bar. If this bar is held by the observer in a horizontal direction behind the theodolite and at right angles to the magnetic meridian, it causes at this distance (about 18.04 feet) a deviation of about 1 minute in arc, which deviation is westerly if the north pole of the controlling bar points eastward, or vice versâ, as the case may be. This deviation, however, diminishes as the bar is removed from its horizontal position, and, on its becoming almost vertical, disappears entirely.

The observer, therefore, need have no hesitation at letting such a bar stand in this position either behind him, against the wall, or within his reach against the clock case, until he wants to use it as shown in the plate.

Recourse must in magnetic measurements be very frequently had to the controlling bar. It is consequently of importance in conducting these experiments to obtain a facility in its use, and to become well acquainted with the laws of its operation. A separate article will, therefore, in the sequel, be devoted to

developing the rules and laws regulating the various applications and effects of the controlling bar.

In conclusion, it may be remarked, with reference to the situation of the building, that other buildings may stand at no very great distance from it, without exercising an injurious effect on the observations. At Gottingen, for instance, in order to prevent those persons who are occupied with these enquiries from being exposed to great inconvenience, it was necessary that the magnetic apparatus should be erected at no great distance from the Royal Observatory, and the most appropriate spot that was open to our choice was selected for the purpose. The centre of this place, which is occupied by the magnetometer, lies about 196.86 feet to the west of the observatory. At this distance, moderately strong magnets exercise so little influence on the magnetometer erected in the magnetic observatory, that experience shows it is quite immaterial to fit up in a room in the former the auxiliary magnetic apparatus, which is found to be of the greatest assistance in absolute measurements, in order, notwithstanding the variations of terrestrial magnetism, to compare the different observations which, when results of this nature are to be obtained, it is necessary should be repeated in a continuous series.

## *Description of Plate* VIII.

In this plate are shown the separate parts of the magnetic apparatus, with the exception of the clock and the theodolite, the measuring rods, the case, and the torsion and controlling bars.

Some of these do not require any particular description, and some of them are shown on a smaller scale in Plates VI. and VII. The construction of the holder with its raising screw, of the cradle and torsion circle, of the mirror frame and its adjustments, of the weight holder and the weights, call for a more detailed description; and they are consequently figured in this plate in several positions of half their actual magnitude.

Three views are given of the cradle with the torsion circle and inserted magnet; namely, as seen from west, from south, and from above; the same is done with respect to the mirror frame. The holder with the raising screw are shown from two sides; west and south.

The south view of the cradle, with the torsion circle and inserted magnet, has been employed for showing the manner in which the weight holder is to be laid across the magnetic bar inserted into the cradle in a direction running east and

west, and how the two weights of 1.102 lb. each are to be suspended therefrom from the projecting points when conducting measurements in absolute intensity, which require a knowledge of the inertia of the oscillating parts of the magnetometer.

To save room in the plate, the two views of the holder and raising screw are placed side by side on the upper line. This arrangement has, however, the disadvantage of removing both these figures out of their proper places with respect to the oscillating parts of the magnetometer appended to them. We see, however, without difficulty how the view of the holder and raising screw given in fig. 1, and the view of the cradle, the torsion circle, the magnetic bar, and the mirror frame as shown in fig. 3, are to be supposed in connexion; for we observe that in fig. 1, is shown the commencement, and in fig. 3, the end of the line by which they are united. The principal parts of the magnetometer are represented in these two figures as seen from the west. In a similar manner figs. 2, and 6, are to be taken together, and represent the instrument as seen from the south. In fig. 6, the mirror frame has been removed from the south end of the magnetic bar, as it would conceal the cradle which is situated behind it.

A separate view of this mirror frame is given in fig. 4.

In the west view seen at fig. 3, only the little notch in the cradle into which the weight holder is let is given, but in the southern view as shown in fig. 6, is seen not only the weight holder as it is inserted into the notch and lying across the magnetic bar, but the two weights are also shown as they are to be suspended from the projecting points.

Fig. 1, represents the holder with the raising screw and thread as seen from the west. A A is a board fastened to the ceiling. B B are two parallel wooden rods glued on to A A, between which a slide D D supported on two projecting shoulders moves backwards and forwards from east to west. The brass collars E are fastened to the slide by screws, and in these collars the raising screw works, running east and west. F is the westerly end of the screw, and in this view conceals the screw itself. G is the thread attached to the screw.

Fig. 2, shows the holder with the raising screw as seen from the south. A A here represents a longitudinal view of the board fastened to the ceiling. B B is the north rod glued thereto. C C the shoulder supporting the slide and having a scale affixed to its edge for giving C C its proper position. D D is a longitudinal view of the slide to which the brass collars E E′ are screwed, and through which the raising screw,

whose head is F, passes. The worm of this screw turns in the grooved collar E, and is kept in its place in it by the nut H. Near the second collar E′ the screw becomes a plain cylinder passing through the smooth aperture made therein for its reception. The line G is fastened to the end of the thread of the screw, and lies in the worm of it till it comes half way between the collars; it here hangs down perpendicular and has the cradle of the magnetometer connected with its lower end. If the line has to be taken up, the nut H must be loosened and the screw is to be turned round in the collars by the head F.

Fig. 3, represents the oscillatory parts of the magnetometer as seen from the west. It consists of two hooks A A, the hindermost of which is in this figure concealed by the front one. The lower end of the thread G is fastened to a pin that catches against the bottom part of these hooks. This portion of the magnetometer comprises also the torsion circle B B, upon which the cradle C C C C rests, the magnetic bar D D, the mirror frame E, with the two smaller frames F F, and H H, and the clamps K K for the reception of the mirror. With the exception of the magnetic bar which of itself weighs 3.75 lb., and the mirror which must be made of tolerably stout glass so that its surface may be truly plane, all parts of the magnetometer are made of thin brass, so as not materially to augment the inertia.

The line supporting the cradle is not fastened directly to it, but is connected to a pin catching below the hooks A A, so that it may be detached without inconvenience.

This pin is furnished with two points about 1.57 inch apart, and which fit into two small notches in the hooks A A.

The torsion circle B B carries a vertical peg, to whose upper end the hooks are fastened, and across which they meet, allowing of a horizontal motion round it as a centre. The cradle itself reposes on the periphery of the torsion circle, the friction upon which, however, retains it in its place. At the extremity of the magnetic bar D D is seen the mirror frame, which, at E, is formed into a kind of sheath, enclosing the bar to which it is attached by binding screws. One of the smaller frames allowing of a vertical motion round the axis of the bar is seen at F F′. At the farther side of this little frame, and therefore hidden from our view, is a small binding and adjustment screw for regulating its position. In connexion with this little frame F F′, there is a second little frame H H moveable at F′ about a horizontal axis, the position of this second little frame with relation to the first one, by means of the screw and nut shown at the upper part of the figure.

To this second little frame are fixed the clamps intended for the reception of the mirror. These clamps are three in number: in this figure, however, only two of them are visible, the third being hidden by the second at K′.

Fig. 4, gives a more detailed account of the mirror frame and its various parts, which are more distinctly seen in this view from the south than in the above one from the west. The various parts are here indicated by the same letters.

The rectangle in the back ground between E and E″ is the section of that sheath-like part of the frame that slips into the magnetic bar, and to which it is to be screwed fast. This sheath has on one side two projections E′ and E′, forming the vertical (in our sketch the horizontal) axis of the little frame F F′ F′ F′.

Towards the opposite side by E″ there is a third projection against which the screw and nut work for altering and maintaining the position of the frame in question. To this first little frame there is at F′ F″ adapted a horizontal axis (vertical in our sketch) about which the second little frame H H H H moves. Facing this axis there are in both these little frames small projections serving, as in the former case, to alter and secure the position of this second little frame by means of a screw and nut. Three slits H H, H H, H H, are, it will be observed, made in this second little frame and in which three small slides may be moved up and down. This arrangement serves for regulating the spaces for receiving the mirror according to its size. The south ends of these little slides terminate in three small vertical studs, upon which the edges of the back of the mirror are brought to bear, while the head of a screw, whose worm goes close to the edge of the glass and turns in the slide, presses down upon its front surface.

In this figure the slides themselves are not shown, but simply the heads of the three screws that work into them, and by which they are entirely concealed.

After this detailed account of the first figures, a few short remarks will suffice for those that remain.

Fig. 5. In this view of the cradle, the torsion circle, magnetic bar, and mirror frame as seen from above, the torsion circle presents itself very distinctly; as does also the shape of the cradle lying on its circumference and moveable on it like a vernier. We likewise see the double hook with its two notches attached to the peg, springing from the centre of the circle. The brass pin catching into these notches has been omitted for the sake of clearness. We also see in this figure how the mirror is fastened into its frame.

Fig. 6. In this figure, to which frequent reference has been already made, it may be particularly observed how the pin to which the thread is attached catches with its two hooks which are connected by a cross plate, in which there is made a rectangular hole; this cross plate being fitted on to the peg in the centre of the torsion circle, which is filed square for that purpose, and on to which it is screwed fast.

As the cradle and magnetic bar must be lifted up when the latter is to be inverted for ascertaining its magnetic axis, the pin to which the cradle is fastened would fall out, were not a small spring adapted to it underneath. This spring, which serves to keep the pin in its place, may be seen in the figure.

The wooden rod of upwards of 27.559 inches in length, which in this figure is laid across the magnetic bar, and to which the two weights of 1.102 lb. are suspended to increase the inertia, is provided with six points, on which the two weights can be hung at three different distances. The two points nearest the centre are 3.3937 inches apart, the next 15.748, and the outside ones 27.559. The first and last are stationary, but the intermediate ones can be taken out and inserted into other holes at intervals of 1.96 of an inch.

The distances between all these points must be laid down with microscopic accuracy.

Figs. 7, 8, and 9, represent the pin to which the thread is attached, as seen from the side, from above and from below.

The first gives a sketch of the two points with which this pin catches into the notches in the hooks on the torsion circle, and likewise of the spring that keeps this pin in its place, even when the cradle is held up and the thread loosened from the pin.

The second shows the small circular hole through which the thread is drawn, and by which it is kept pressed tight.

The third shows an oval opening bisected by a round cross pin. The thread is bent round the latter and pulled tight, having been previously drawn in its whole length through the loop formed at its lower end.

Fig. 10, shows, in conclusion, a specimen of the scale placed beneath the theodolite, its image being observed through the telescope of this instrument. When employing an astronomical telescope, which is to be preferred to a terrestrial one from its presenting the advantage of greater clearness and shorter focal length, the apertures being alike, the scale must be inverted; in which case the figures would stand underneath the divisions of the scale instead of being, as in our figure, above them.

*Expenses incurred by the building and fitting up of a magnetic observatory.*

The expenses of a magnetic observatory may be comprised under two heads: namely, those of the building and those of the instruments.

The expense of erecting the building will vary at different places. By way of example, it may be remarked, that the builder's account of the magnetic observatory of Gottingen, complete, amounted to £113.

It must be observed, however, that a portion of this expense was incurred by entirely excluding iron from the building. All nails, locks, window and door hinges; every thing, in short, that is usually of iron, is here made of copper; by which means there is within a considerable range of the magnetometer nothing that can interfere with the accuracy of its indications.

The following are the prices of the instruments as extracted from the catalogue of Mayerstein, Optician at Gottingen, from whose workshops most of these instruments now in use have proceeded.

| | | £. | s. | d. |
|---|---|---|---|---|
| 1. | An eight-inch theodolite - - - - - | 21 | 10 | 0 |
| 2. | Clock, beating seconds - - - - - | | | |
| 3. | Firm stand for theodolite - - - | 1 | 0 | 0 |
| 4. | Scale with its setting - - - - - | 0 | 3 | 0 |
| 5. | Illuminating apparatus - - - - - | 1 | 12 | 0 |
| 6. | Holder with slide and screw - - - | 1 | 3 | 0 |
| 7. | Cradle and torsion circle - - - | 2 | 3 | 0 |
| 8. | Principal bar, weight 4 lbs., box for it, 4 lb. auxiliary bar, and 1 lb. controlling bar | 1 | 0 | 0 |
| 9. | Brass torsion bar, with magnet let into it - | 1 | 6 | 0 |
| 10. | Two mirror frames, with adjustments and mirrors - - - - - - - | 6 | 3 | 0 |
| 11. | Weight holder, and two weights of 1.102 lb. each (½ kilogramme) with arms - - | 1 | 0 | 0 |
| 12. | Case, with glass lid - - - - - | 2 | 6 | 0 |
| 13. | Three measuring rods, 19.828 feet long, with stands - - - - - - | 0 | 12 | 0 |
| | | £39 | 18 | 0 |

JULIAN GUGGSWORTH.

*Wormwood Scrubs,*
*June* 23, 1838.

## XX. *On the Propagation of Electrical Currents through Liquids.* *By* M. Charles Matteucci.*

The passage of electrical currents, through conducting bodies, either solids or liquids, has been for a long time the subject of most profound and extensive research, and much celebrity has been gained by scientific men of our age, as Arago, Ampère, Becquerel, Faraday, De la Rive, &c., for discoveries made in this branch of electrical science. But the mine is far from being entirely explored, and the efforts of scientific men on this subject never cease being crowned by some important discoveries.

This work is the fruit of research, commenced three years ago, and has been pursued constantly: I have tried in the course of my experiments to find some remedy for the defects of my experimental method, and thereby render it less unworthy, if possible, the consideration of the learned. I will explain more clearly the object of my labours.

Whenever the current of a pile is transmitted by a conductor, partly metallic, and partly liquid, we can, by several methods, modify its intensity (by the intensity of the electric current I mean its action on the magnetic needle of the galvanometer) without attaching to it any "theoretic value." We acknowledge two different orders; the first relates to the strength of the pile, the other to the nature of "reophore" (by reophore I mean the assemblage of electrodes and of the liquid conductor). The elements which constitute the strength of the pile are the number of pairs, the extension or the surface of these pairs, and lastly, the nature of the liquid between them, as far as regards its conductibility and chemical action on the metals. With regard to the elements of the reophore, we have— 1st, the nature or chemical composition of the liquid; 2d, its temperature; 3d, its volume; 4th, the extension and the nature of the electrodes. It is the influence of the last named elements of the reophore separating and re-uniting together on the intensity of the current, that I propose to myself studying, modifying at the same time the force of the pile, and by that the original intensity of the current.

Afterwards I shall endeavour to determine the influence of metallic diaphragms, and liquids placed in the route of the current, also its intensity. I have finished my work by researches on the modifications brought on the intensity of the current; following the direction in which this current runs through the reophore.

* Communicated by the Author, and translated from the French by Miss Bachhoffner.

Before commencing an explanation of these researches, I will say a few words on the manner and method of observation. The piles I made use of were of plates of zinc and copper. I employed them of different dimensions, and disposed of them sometimes in columns, sometimes in troughs.

I was particular in remembering every time I described the phenomena observed, with which of the piles the current was produced. During my first researches I made use of a pile that I called a constant force. The construction is very much the same as that employed by M. Daniel, and as this learned gentleman has been before me in his publication, I will say but few words on the subject.

This last analysis is but the execution of an idea published some time since by M. Becquerel. This pile of "constant force" is composed of a series of little rectangular boxes of varnished wood, every one cut in two, in such a manner as to form three cavities. Two plates of copper are placed in their two extreme spaces; these are separated by means of a piece of membrane. The middle space has, at the bottom, a hole into which passes a tube of glass, which reaches the bottom of the box, and terminates in a tube almost capillary. Another rectangular box, also of wood, and rather longer, shuts over the edge of the boxes of the pile: there is at the bottom a series of holes corresponding with the spaces of the pile, and these holes again terminate by tubes of glass, the same as those of the boxes of the pile. The extreme cavity of every box is full of saturated solutions of sulphate of copper. In the middle one is the sheet of zinc, amalgamated on its surface, plunged in a solution of sulphuric acid. This same solution is poured in the principal space; and as the running is equal, or nearly so, for all the tubes, one can easily conceive the manner in which the liquid that moistens the zinc is renewed. The plates of copper do not suffer any chemical action on the part that is saturated by the solution of sulphate of copper, in which they are immersed. The intensity of the current is known by the galvanometer, and I have been obliged sometimes to take notice of the primitive deviation. When arcs are made use of, in part liquid, and good conductors, the intensity of the secondary currents are so great, that one cannot always attend to the fixed deviation;* they diminish

* This primitive deviation is the effect of the impulsive movement, by which the speed communicated to the needle is proportional to its intensity from 0°, until the whole arc is described, and certainly cannot be expressed in the same manner as the forces which produce the steady deflection, in which case the current is expressed by the tangent of the arc: whereas in the other case the force is expressed by the chord of the arc of primitive deflection; in the

down to zero, and even produce an inverse current to that of the pile.

These instruments have more or less sensibility, and were constructed in the ordinary manner. When I wish to study the relative conductibility of a certain "reophore" I have employed the galvanometer of double threads. The table of intensity was determined by the method described by M. Becquerel.

The electrodes were, in general, plates or wires of platina. Great care must be used to keep these plates polished; to effect it they are immersed in a solution of sulphuric acid rather warm, and then washed several times in distilled water. It is also necessary to protect them from those secondary "polarities," which are so easily developed on the platina. I have employed several methods for this purpose that have quite answered my expectations; in the first place only four or five minutes elapsed between each experiment to see almost completely disappear these secondary currents, particularly when they consist of currents originally weak, and liquid reophores that are very imperfect conductors. You may succeed still better by uniting by a metallic thread the two electrodes after the passage of the primitive current. The circulation of the secondary currents is settled, and the "polarity" disappears by that the more easily. It can be done another way, and this method has also succeeded very well. Make the original current pass in a direction relatively opposed to the electrodes, and for the same time, and the secondary polarities are destroyed instantly. In general I have not made a second passage of currents, without assuring myself beforehand, that the secondary polarity had disappeared. With the other piles that were not of "a constant force" I was always particular in noticing that the strength of them was not sensibly changed.

I have studied the effect the membranes have produced that I employed in the course of my experiments. When they are fresh and have been plunged into the liquid during half an hour, they cease to weaken the current and oblige it to pass through; they prevent the mixture of liquids during a sufficient time for observation in experiments of this kind. I think it useless to describe all the other means employed in these experiments: they are known to all scientific men accustomed to this kind of research. I propose to myself studying in what

same manner as pendulous motions. Having ascertained, by a great number of experiments, that the same arc of primitive deviation is produced when the force of the pile is constant, and no change is made in the connecting circuit, one may make use of this primitive deviation to compare the intensities of different currents.

manner the electric current, measured by its action on the magnetic needle, is modified in its passage through liquids, by the different strengths of the pile and by the variable nature of the reophore. I ought again to examine the nature of the electric current, and I will explain in a few words the actual state of our knowledge. We know positively that the electrolytical power of a current depends, in an elementary pile, on the intensity of chemical action it develops, whilst the quantity of this action is derived from the quantity of metal beneath, or from the quantity of electro-chemical action generated. Thus it is that if a current has not, by defect of chemical action, a certain degree of electrolytical power, it cannot be given to it by augmenting its quantity.

M. de la Rive admits, also, that all chemical action gives to the current that it develops, an individual electro-chemical character. We know that by increasing the number of pairs of a pile nothing is added to the quantity of electricity developed. Thus it is, that the magnetic action of the current of a pile does not differ in any one of these pairs, not even the weakest, if the current of the pile is entirely discharged by a metallic conductor sufficiently thick for the electrical charge to pass through. When the arch is not perfect, by one portion of the current of the pile taking the road of the arc, and the other being discharged by the pile itself, you will, by adding to the number of pairs, render the first method of circulation worse. The greater part of the current passes by the exterior arc, and its action on the magnetic needle and electrolytical power is by that augmented.

The relation between these two complimentary quantities depends on the relative conductibility of the pile and the arc, and at the same time, by the same rule, the force and construction of this pile. We know also that all electricity developed by chemical action does not circulate. There is one part that is recomposed on the surface of the positive metal, and this part often varies the conductibility of the intermediate arc. It is by these means that hydrogen is developed on the surface of the zinc, whilst in circulating currents, hydrogen is developed on the electro-negative metal. I shall begin by stating a fact that will throw some light on the nature of electrolytical power. I have been able to obtain electrolytical decomposition when the current passes through liquids by very narrow eloctrodes, whilst this same current does not give place to any decomposition if the electrodes are very extended.* These experi-

* This work had been finished some time when I read that De la Rive, for the single electro-magnetic currents, had arrived at the same conclusion, and that Mr. Sturgeon was occupied with the same subject.

ments appear to me interesting enough to merit being described with the utmost precision. My pairs were composed of a square plate of amalgamated zinc, which measured $0^{m}$ 03 of each side; the surface of copper was double, and bent the same as in the pile of Wollaston; the liquid of the pile was a solution of sulphuric acid in rain water, in the proportion of one part acid to a 100 or 110 of water. To the two plates were soldered two wires of copper which were continued to the electrodes of platina; the most extended were 5 cc., the small ones were $0^{m}$ 003 broad, and discovered from 8 to 10 millimeters.

I again prepared a solution of hydriodate of potassa contained in two capsules; by one of these capsules the current is transmitted by the broad electrodes, in the other by the narrow. Before the passage of the current commences, I drop in the hydriodate some drops of a solution of starch. Some few minutes after the current circulates, the narrow electrodes are covered with a dark blue foam. On the broad electrodes nothing of the kind is perceivable; not even after having prolonged during six hours the passage of the current. I wish to observe that if the current has but a single passage to make, the decomposition is produced equally with the broad and narrow electrodes. I have tried to electrify another liquid. It was a saturated solution of sulphate of copper at $+ 20°$ R. The current of a single pair even after a very prolonged passage does not give place to any decomposition, when it has only a single passage through the liquids. Two similar pairs united give decomposition with the extended electrodes, and with the narrow ones when there is only a single passage. If the current passes in the two liquids at the same time, there is no decomposition except on the narrow electrodes. I have again electrified the solution of sulphuric acid the same that excited the current. With a single pair I obtain no trace of decomposition on any occasion whatever. With two pairs I observed nothing with the two passages; but decomposition commences if the current passes by the single narrow electrode, but not with the broad. With three pairs the decomposition does not take place if the two passages are united; it occurs, however, with the broad and narrow electrodes when separately employed. Lastly, four pairs produce decomposition on the little electrodes and not with the broad. I observed that all these experiments are prolonged by renewing the acid of the pile, in the manner I have described.

The observations I have made suffice to establish that the electric current, discharged in a liquid, when it is transmitted by little electrodes, is endued with an electrolytic intensity much greater than that it would have if it was transmitted by

more extended electrodes. It is worthy of notice that whenever the action is measured on the magnetic needle, it is found to be stronger for the current that is discharged by the extended electrodes; thus it is in the solution of hydriodate of potassa, there are 10° with the narrow electrodes, and 50° with the broad. It is to be observed that this difference of deviation is not prolonged after the first moments of the passage of the current. Again, I wished to determine what was the quantity of zinc dissolved in producing these different currents. The passage of the current was prolonged in every experiment for an equal time. The zinc was weighed before and after the experiment. I think it unnecessary here to name the number of experiments I have tried, to arrive at these general conclusions; suffice it to say, whenever the elementary current is obliged to pass through a liquid, the bubbles of hydrogen always show themselves on the zinc, and these bubbles increase in number when the liquid, traversed by the current, becomes electrolized. One has only to make a metallic communication between the zinc and the platina of the pile, to see appear at the same instant, a torrent of bubbles of hydrogen on the surface of the platina, and those of the zinc completely disappear. I shall show these results in the following conclusion:—

1st. When the current of an elementary pile composed of a plate of zinc amalgamated on its surface, and of a double plate of platina, circulating by a metallic arc; the hydrogen only is disengaged from the surface of the platina, and the quantity of zinc dissolved in this case is much greater than that found to be when the current, developed under the same circumstances, is circulated by an arc, in part metallic in part liquid. Thus it is that 135 millig. of zinc are dissolved in the same time as 12 millig.; the liquid of the pile being water acidulated by sulphuric acid in volumes of 300 parts water and 5 parts acid. In the first case the arc was all metal; in the second there was a bed of rain water to traverse.

2d. When the elementary current passes through a liquid, the amount of electro-chemical action generated, measured by the quantity of zinc dissolved, depends principally on the degree of electrolization with which it is endowed; that being accomplished by the variable extension of the electrodes, and by the nature of the liquid traversed by the current. Thus it is when electrolization is produced by the little extension of electrodes, or by the easy electrolization of the liquid; it will be found in both these cases, the quantity of zinc dissolved is much greater than it would be if the passage of the same current was through broader electrodes, or with a liquid less easily electrolized; in the which case there would be no decom-

position. I have made ten successive experiments with the same arrangement of apparatus, plates of zinc of equal surface equally amalgamated and very little difference in weight from each other, the acid liquid was the same as that I have before described. In five of these experiments the current was circulated by electrodes twenty times more extended than that of the other five. In the first the liquid conductor was of rain water, and the galvanometer marked 90° of primitive deviation; in the others the liquid was a solution of hydriodate of potash in 1-800 of distilled water, and the galvanometer marked 10°. The circulation of the current was maintained during thirty minutes, and the amount of zinc dissolved was for the first 4 millig., and 10 for the others.

3d. If in any case the current is made to circulate by extended electrodes, and in another by very small electrodes, employing a liquid conductor capable of being electrolized; in both these cases it will be found that the quantity of zinc dissolved is much greater, if it has magnetic action, when the current circulates by broader electrodes.

4th. This same result is verified when the electric current is transmitted by electrodes of a very variable extension, without there is electrolization, slightly apparent. Therefore, if you make the current of an elementary pile pass through rain water, by very extended electrodes, and another current equally developed by very small electrodes, and in the same liquid, you will not perceive any electrolization in either of these cases, and the quantity of zinc dissolved is greater in comparison for the current passing by extended electrodes. In general, where there is no electrolization, the quantity of zinc dissolved in generating the current diminishes, on all occasions, that it weakens its magnetic action. It often happens by a variable time in the settling of the liquid, or by the different nature of this liquid, that it is not rendered electrolizable.

5th. When, instead of an elementary pile, several pairs are united together, it is very true that the quantity of zinc dissolved by every plate of zinc when they are united, is equal to that obtained if every pair was separately employed. It will be necessary for a true result that the current of a pile or of a pair is calculated by a conductor wholly of metal.

If, on the contrary, the current be transmitted through a liquid, and in this case the electrolization is stronger by the pile than by the elementary pair, the quantity of zinc dissolved is more for the united pairs, every one taken separately, than for one of them employed alone in the same circumstances. I have taken four plates of zinc and I united them in piles, and the

current passed by electrodes of platina in water slightly acidulated; four other equal plates, I used in this experiment with small electrodes, which produced in both cases electrolization. The amount of a great number of experiments gives 20 milligrammes of zinc dissolved for every plate of the pile of four pairs, and 16 milligrammes for a plate employed in an elementary pile.

It appears to me very easy to conceive how quantity gives electricity in passing in a liquid by narrow electrodes can produce a stronger electrolytical effect than that it would by being introduced by more extended electrodes. The quantity that presents itself in each thread of liquid that touches the electrodes, is much greater in one case than in the other, which is possible even when the quantity of electricity circulated is very small.

I cannot admit that every chemical action generated by electricity gives an individual electrolytical character. This electrolytical force or power appears to me to depend on the quantity of electricity developed in reference with the quantity circulated and with that which presents itself at the same time, to a certain thread of liquid united together.

## CHAPTER I.

*On the influence of the nature of liquid on the intensity of the electrical current transmitted.*

Independently of the electro-chemical decomposition which accompanies, in most cases, the passage of the electrical current through liquids, and which certainly exercises a very great influence on the conducting power, there is for these bodies the same power of transmission of the electricity that is proper in a superior degree of the carbon, and above all, of metals. One body only, the "periodure" of mercury, is capable of conducting electricity without being decomposed, suffices to establish this principle. We see, also by the following researches, that bodies very easily decomposed by the electric current, are very far from possessing the same power of conducting as others, by which the passage of the current is made without any sensible electro-chemical decomposition. The research of the electrical conductibility of liquids was undertaken M. M. Gay Lussac and Thénard, and afterwards by M. Marianini, and at length by myself.* It must be

* M. Pouillet has just published a very important work on electric currents. He describes in this work, which is very little known at present, a very ingenious process for comparing the conductibility of liquids and metals.

acknowledged that the methods employed by these different observers is very far from being exact and complete. In the researches of M. Marianini, the conductibility was determined by plunging in different liquids a voltaic pair, and in making observations by the galvanometer of the current developed. One can easily see, as is well observed by M. Becquerel, that in this method account cannot be taken of the different intensities of chemical action, and modifications brought on by the new combinations it produces. The method I have myself employed, although exempt from the fault I have reproached M. Marianini with, was not proper to give at the same time the account between the conductibility of a liquid and that of distilled water, to which they have reference. The pile I employed was very far from giving a current of an equal force as required in this kind of research. The results I am going to mention were obtained with a galvanometer of double thread with my pile of equal force and four electrodes of platina, united two and two together, and plunged in the liquid of the same quantity of their surface. The first question I propose to resolve is, to know if the conducting power given to water by the addition of a great number of substances, depends on the conductibility with which these substances are endowed, liquified by heat. I flatter myself with having succeeded in deciding this question; in demonstrating in effect this power is owing to the conductibility acquired in the watery solutions being always the same power modified by the dissolvent. We also know by the researches of M. Faraday that certain substances although liquids by heat never acquire any conducting power for the electrical fluid. These same substances, dissolved in any proportions in water, do not appear sensibly to augment the conductibility; sugar is the principal substance used in cases of this kind. It is very true that other substances singly, although liquified, "following Professor Faraday," can augment the conductibility of water in which they are dissolved, this is the "periodure" of tin; but in this case, it is to the formation of "hydriodic acid" that it is due. I have again determined with correctness the conductibility of certain melted substances, and have directly compared them to those of their solution in water. Nitrate of potash melted gave 42° forthwith, directly after 44°; it increased to 55°, being 2° more than that of distilled water. The acétate of lead gave two different conducting powers; in its first aqueous fusion the conductibility stopped at 16°; in the second fusion it amounted to 30°. We must now look at the conductibility of the solutions of these two salts in distilled water. At first sight I cannot understand how a small quantity of salt can give to a large quan-

tity of water the same conducting power, as melted salt. We shall see directly how this conductibility is always increased by the largest quantity of salt that is dissolved in water; I have then prepared a solution of acetate of lead saturated at + 20° R, and I obtained precisely 16° of conducting power. The saturated solution of nitrate of potash only gave 30°. The result shown by the "acétate" of lead is evident. This salt in the first fusion can be compared to its saturated solution in the water. The chlorure of calcium that I afterwards submitted to the experiment confirmed the preceding result. A solution of this salt saturated at + 20° R. gave 44°; in its aqueous fusion 45°. In continuing to warm it, its conducting power diminished, and when it began to solidify its power was reduced to 35° and at length to 30°. It may be seen by these experiments that the conducting power of melted salt increases or weakens by the action of dissolving. I note here that no electro-chemical decomposition was produced in these experiments. To show still more clearly the influence exercised by dissolving liquids on the conducting power of bodies dissolved, I will mention an experiment of a saline solution made in alcohol. I dissolved 1-100 of sal ammoniac in alcohol, its conductibility was of 6°; I added another 1-100 of sal ammoniac and 1-100 of nitrate of potash and the conducting power was scarcely increased. To this same solution I again added 1-100 of nitric acid and the conductibility became 14°. These same substances added to distilled water in the same proportions gave 25°. I will again give three examples of the power conductors have shown both in the state of saturated solution in water and in that of the aqueous fusion. The sulphate of zinc melted gives 43°, and the saturated solution + 20° R gives more than 42°. The melted sulphate of alumine and potash gives 42°, and the saturated solution 42°. The chlorate of potash has been before tried, when gently heated. It gives 40° when it is completely melted. In continuing to heat it, the conductibility increases and mounts to 47°, and when a crystalline substance is visible at the bottom of the liquid, the conductibility arrives at the maximum of 55°. The solution of chlorate of potash in water saturated at + 20° R gives rather more than 38°. It is easily seen in this change in the conductibility of chlorate of potash the same phenomenon that we have observed in the acétate of lead. We may conclude in general, that *the conducting power of any salt whatever in the watery fusion is the same as that of the solution saturated in water* +20° R. *The proper conductibility of saline matter that obtained in the action of fusing is modified by that of dissolving, and in general it is found to be diminished.*

The difference of temperature in the which the saturated solution is found +20° R, and the salts in their fusions, "aqueous" and "calorific," merit observation. We shall see by what follows that heat has (on the conductibility) a much less influence when the solutions are better conductors and the temperature more elevated. It may not be amiss to observe that the conducting power for every body once well determined, one can easily apply those data to determine the true elementary disposition of certain combinations, that we call the "rational formula." At the same time some light can better be thrown on the question of "hydro-chlorates" or chlorures.

I have now to show in what manner the conducting power of watery solution is modified, by the different quantity of the substance dissolved. I have already shown, in another work published in the Universal Library of Geneva, that this conducting power is not proportionably augmented to the quantity of matter dissolved in water. You have only to reflect on the experiments that I have shown, to see this principle confirmed. All these saturated saline solutions at +20 R, have the same conductibility as the salts in their watery fusions. An example of the same kind is again offered by the three mineral acids—sulfuric, nitric, hydrochloric. Sulphuric acid was tried at three different densities, and has given the following results;

| Density. | Conductibility. |
|---|---|
| 1.8500 | 35° |
| 1.022 | 37 |
| 1.010 | 37 |

It can be seen by this table that the sulphuric acid, the most concentrated, almost anhydrous, is endowed with a conductibility weaker than that it would acquire when it is mixed with a certain quantity of water.* With the other acid the conductibility varies in the following manner:

| Acid Nitric. | | Acid Hydrychloric. | |
|---|---|---|---|
| Density. | Conductibility | Density. | Conductibility. |
| 1.18 | 57° | 1.260 | 58° |
| 1.029 | 47 | 1.027 | 50 |
| 1.015 | 37 | 1.015 | 57 |

The same result is again demonstrated by the hydrochlorate of ammoniac; here are the results:

* This property, of the sulphuric acid, has already been established by M. De la Rive.

| | | | |
|---|---|---|---|
| 10.0 | of sal ammoniac dissolved in distilled water. | 12° = | 18 Int. |
| 20.0 | - - - - - - - - - | | 22 |
| 30.0 | - - - - - - - - | | 26 |
| 40.0 | - - - - - - - - | | 27 |

I will name the results obtained with solutions of the sulphate of zinc.

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| 10.0 | sulphate of zinc | 3° | | Int. | 50.0 | sulphate of zinc | 10° | 13 | Int. |
| 20.0 | - - - - - | 5 | | „ | 90.0 | - - - - - | 12 | 17 | „ |
| 30.0 | - - - - - | 6⅓ | | „ | 70.0 | - - - - - | 13 exc. | 23 | „ |
| 40.0 | - - - - - | 8 | 10 | „ | 80.0 | - - - - - | 14 | 24 | „ |

It will take too long time here to detail all the experiments that I have tried on this subject, which will be found in the following conclusions :

1st. The conductibility of a watery solution increases in general by fresh doses of the substances dissolved.

2d. There is, however, a limit at which this conductibility, by fresh doses of saline substances dissolved in the water, ceases to increase. With piles endowed with only a weak power of production and propagation and equal weight of the substance added, this limit is attained sooner for the substance which communicates the greatest conducting power.

3d. When the pile is endowed with a very large conducting and propagating power, the conductibility of the solution varies proportionably to the quantity of saline substance dissolved, and that in a greater proportion for the substances which gives to water a greater conductibility.

4th. The limit at which the conductibility of a solution is arrested by fresh doses of the substance dissolved in water, is thus removed much farther as the force of the pile is greater, and much greater is the conductibility communicated to the water by the substance dissolved.

5th. We shall see presently, in studying the influence of the number of pairs, that it ceases sooner with weak piles, in a good conducting liquid, whilst the contrary happens if the pile is endowed with a great power of production and propagation.

I now proceed to results obtained in studying the conducting power of several salts or bodies whatever, melted or dissolved in water, and mixed together without there being any decomposition.

I begin by trying the conductibility of melted salts. I employed for that purpose the acetate of lead, the nitrate of potash, the sulphate of zinc, and the double sulphate of alumine and of potash.

The general result at which I have arrived, is, that *the conductibility of a mixture of several melted salts, does not*

In the following table I will report the results obtained, in taking for the liquid of the pile rain water, and obliging the current to pass through the four following liquids: A = distilled water, B = solution of 1-100 of sulphate of zinc in distilled water, C = saturated solution of sal ammoniac at + 20° R, D = solution of sulphuric acid.

| Number of pairs. | A | | B | | C | | D | |
|---|---|---|---|---|---|---|---|---|
| | | Int. | | Int. | | Int. | | Int. |
| 10 | $\frac{1}{6}$° | „ | $2\frac{1}{2}$° | „ | 14° | 24 „ | 16° | 28 „ |
| 20 | $1\frac{1}{2}$ | „ | 5 | „ | 17 | 30ex. „ | 18 | 36 „ |
| 30 | 2 | „ | 7 | 8 „ | 18 | 36 „ | 15 | 26 „ |
| 40 | 2 ex. | „ | 8 | 10 „ | 15 | 26 „ | 14 | 24 „ |
| 50 | 3 | „ | 7 | 8 „ | 12 | 17ex. „ | „ | „ „ |
| 60 | 3 | „ | 7 | 8 „ | 11 | 16 „ | „ | „ „ |

I think it useless to name here any more experiments; they would be similar to those already described; we can draw from them the following conclusions.

1st. The action of the current on the magnetic needle propagated by different liquids, more or less conductors, increases in general with the number of pairs, and in a larger proportion for bad conducting liquids.

2d. There is a limit in the number of pairs, which produce, in a certain liquid, the current that exercises the maximum of magnetic action, and this limit arrives sooner for the good conducting liquids than for the bad; thus it is in the solution of sulphuric acid, the pile of twenty pairs gives the maximum current, whilst this maximum arrives with sixty pairs in distilled water.

3d. In passing this limit in the number of pairs, the intensity of the propagated current begins to weaken.

4th. The results are not true with a pile, endowed with only a weak power of production and propagation.

When, instead of a liquid of small conducting power and active in the pile, they make use of a solution of nitro-sulphuric, the results are very different from those I have just shown. Here is another table obtained with these acid liquids in the pile. The pairs were the same: the conducting liquids were A = of distilled water, B = a solution of 1-100 of sal ammoniac, C = 8-100 of sal ammoniac, D = saturated solution of sal ammoniac.

| Number of Pairs. | A | | B | | C | | D | |
|---|---|---|---|---|---|---|---|---|
| | | Int | | Int. | | Int. | | Int. |
| 8 | 1° | " | 7° | 8 " | 13° | 23 " | 38° | " |
| 16 | 2 | " | 14 ex. | 25 " | 34 | " " | 60 | " |
| 24 | 3 | " | 22 | " " | 48 | " " | 84 | " |
| 32 | 5 | " | 28 | " " | 60 | " " | " | " |
| 40 | 6 ex | " | 33 ex. | " " | 74 | " " | " | " |

One can easily see by these experiments that when the pile is endowed with a great power of production and propagation, the intensity of the current increases proportionably to the number of pairs, and in a much larger proportion than the conducting liquid is endowed of a greater conductibility.

We are conducted to these conclusions when we examine the influence of the extension of pairs. I will only show here two tables. The pile was of columns of 10 pairs. Its liquid was of rain water. The current passed by two liquids; one was a solution of 1-100 sulphate of zinc, the other a saturated solution of sal ammoniac. In the second table will be found united the result obtained, in taking for the liquid of the pile a solution of nitro-sulphuric.

*1st Table.* *2d Table.*

| Extension of the Plates. | 1st Table A | | 1st Table B | | 2d Table A | | 2d Table B | |
|---|---|---|---|---|---|---|---|---|
| | | Int. | | Int. | | Int. | | Int. |
| 1=0m 05 diam | 4° | " | 7° 8 | " | 5° | " | 8° 10 | " |
| 2 united 0m 05 | 4 ex. | " | 12 18ex. | " | 7 | 8 " | 17 30ex. | " |
| 3 " | 6 | " | 17 30 | " | 8 | 10 " | 24 | " " |
| 4 " | 6 ex. | " | 23 | " " | 8 ex. | 10 " | 35 | " " |
| 5 " | 7 | 8 " | 27 | " " | 9 | 12 " | 40 | " " |
| 6 " | 8 | 10 " | 37 ex. | " | 10 | 13 " | 42 | " " |

It can be seen by this table that the intensity of the current propagated through a liquid, augments by the extension of the surface of the pairs, and that in a larger proportion for the good conducting liquid, by reason of the strength of the pile.

I will finish this chapter by uniting in a table, the relative powers of conductibility, with which certain solutions are endowed. I have made use of, for this determination, my pile of constant force of eight pairs, and of the same galvanometer of double thread. All were compared together at the same time, to distilled water. The solutions contained 1-100 of dissolved substance. The temperature was of + 10° R; the solutions were contained in two small boxes of varnished wood, of equal dimensions; they were 0m 20 long, the breadth and depth being each 0m 01. The electrodes were of platina and of an equal surface to that of the bed of liquid.

Here are the tables :—

*1st Table.*

| | | | Int. |
|---|---|---|---|
| | Distilled water | 0°½ | id |
| Solution of bi-carbon, of soda | | 6 | id |
| id | Acétate of morphine | 2 | id |
| id | Bicarbonate of potassa | 5 | id |
| id | Carbonate of potassa | 8 | 10 |
| id | Nitrate of silver | 6 | id |
| id | Iodide of potassium | 3 | id |
| id | Carbonate of soda | 8 | 10 |
| id | Hydrochlorate of amm. | 12exc | 18exc |
| id | id of soda | 10 | 13 |
| id | Carbonate of ammon. | 7 | 8 |
| id | Nitrate of potassa | 5exc | id |
| id | Chlorure of calcium | 5 | id |
| id | Acétate of lead | 2 | id |
| id | Sulphate of copper | 3 | id |
| id | Sulphate of iron | 2 | id |

*2d Table.*

| | | Int. |
|---|---|---|
| Nitrate of potassa | 53° | „ |
| Acetate of lead | 30 | „ |
| Chlorure of calcium | 30 | „ |
| Alum | 42 | „ |
| Sulphate of zinc | 43 | „ |
| Chlorate of potassa | 50 | „ |

With the same distilled water and the same pile I have compared the conductibility of several dissolved salts: they are united in the second table. Having renewed the zinc of my pile, of constant force, without making any other change, I will here show a few of the results obtained.

| | | | Intensity. |
|---|---|---|---|
| Distilled water | | 0° ½exc | id |
| Sal ammoniac | | 17 | 30 |
| Carbonate of potassa | | 9 | 12 |
| Nitrate of silver | | 8 | 10 |
| Id of potassa | fused | 90 exc | „ |
| Alum | id | 80 | „ |
| Acétate of lead | id | 75 | „ |

It can easily be seen the little importance these observations are of, when the modifications are known to be brought in the conductibility of any solution whatever, by the different power of the pile, the number of the pairs, &c.; modifications which are very far from being reduced to precise and mathematical laws.

*(To be continued.)*

## XXI. *Why do electrized bodies recede from one another? By* Charles Griffin, Esq.*

My former paper, read to the Electrical Society on June 2, was intended to show some evidence in favour of homogeneous attraction of Electricity, and that not only on its own account, but to open the minds of philosophers to receive

* Read before the London Electrical Society, Tuesday, July 17, 1838.

with less aversion a new answer to the question at the head of this article, and which answer I purpose to give partly in this paper.

In two or three instances where I have read of a new answer having been proposed to that question, philosophers have shown a proneness to discourage any opposition to the doctrine of repulsion.

My arguments, in this, my second paper, will certainly be rather general, but I trust will not be considered an inappropriate introduction to a third paper, in which I intend to consider, more particularly, the influence exerted by one and two insulated balls, or atmospheric atoms, electrified positively or negatively, on adjacent balls or atoms, and on each other, independently of any repulsive power belonging to electricity or to ordinary matter.

"Having had my curiosity excited by reading something of Dr. Franklin's experiments and discoveries, I last summer (1832) procured the short treatise on electricity, published in 1827 and 1828, by the Society for the Diffusion of Useful Knowledge, and, after a careful perusal of that and such other accounts as I met with on the subject, became a single fluidest and was led to reflect much on the singularity of negative repulsion.

"It seemed evident there was a fallacy in Franklin's theory as regarded repulsion; but I could not for one moment believe it arose from the idea of there being one fluid only. I was also as incredulous as to any repulsion between the parts of other matter; and the one electric fluid being gone, there could be nothing left to exert a repulsive power.

"I therefore concluded, there must be something without the receding bodies that concurred in causing their recession; and that as one state of electricity had always been found to have a tendency to induce the other in the surrounding bodies, that a negative state of electricity in two suspended balls might induce a positive state in the surrounding air. Each ball thereby acquiring a positive state of electricity, the two atmospheres I thought might repel each other, and carry their balls with them as nearly in their centres as the disturbance of gravity would admit.

After some reflection upon this view of the subject, I considered that the non-conducting quality of the air would not permit that rapid transference of electrical particles which is produced by induction; and if it did, that it might as easily permit a transference of electricity from the air to the bodies themselves, and, consequently, that the instantaneous recession of electrified bodies could not arise from that cause. I also

considered that the elastic property of the air would not allow that to be condensed with the electricity round negative bodies, so as to produce a positive atmosphere.

The transition was now easy to the reflection, that if bodies negatively electrified had an attraction for the natural quantity of electricity of other bodies, then two negatively electrified bodies, suspended in the usual way, must exert an attractive influence on the natural quantity of electricity of the air around them; and if the electricity would not approach them, they might approach the electricity. They might, but whether they must, depended upon the equality or inequality of the force of this attraction on each side each ball.

"On the remoter sides of the balls I considered they were attracted by the electricity of the air with greater force than they were attracted on the adjacent sides; supposing that a particle of electricity acted on by both balls attracting it in different directions, would lose something of its force in each, as a magnet which can just sustain a certain weight of iron, can no longer do so on the approach of another piece.

"I also considered, that, although the balls had, by being deprived of their own electricity, acquired an increased attraction for the surrounding electricity, yet that each ball had, in fact, lost part even of its natural portion of attraction on the side adjacent to the other ball, because that other had lost its natural portion of electricity.

"It is found by experiments of Coulomb and others, that in an elongated body electricity is most intense at the ends; and if the body be not very long, the electric intensity is almost nothing at the middle of it; and I judged that the two balls were liable to the same law, and should be considered, while in contact, as one body, with the negative electricity accumulated at the remoter sides, and almost nothing at those adjacent, where it would be nearly in its natural, and consequently neutral, state.

"If then the two adjacent halves of the balls be considered in a comparatively neutral state, they ought to have little more attraction for the electricity of the air on their adjacent sides than they had in their natural state while the negative electricity or deficiency of fluid continually increases in intensity towards the remoter sides of the balls, which are consequently attracted with a great excess of force by the electricity of the air and other bodies on those sides."

I have come to the further conclusion, that positive electricity has no more claim to the supposed quality of repulsion than negative electricity. We have "only to transpose the hypothesis of the recession of negatively electrified bodies

from each other, by substituting excess of electricity for excess of matter in the balls, and considering that excess of electricity to act upon the matter of the air instead of its electricity."

I once thought the recession of the balls of the pith electrometer, when brought in their natural state into an electrified atmosphere, furnished an argument *per se* against repulsion, but I believe now it may be well explained, consistently with that doctrine.

"In considering this question (of repulsion) I have given much weight to the usual simplicity of natural operations. That has induced me to endeavour to do without two fluids, and it has had much to do with making me investigate closely the causes of electrical recession, and ultimately to reject repulsion altogether, as a cause of it. The fact that electrified clouds are not rapidly scattered in every direction, had also much influence in leading me to the latter conclusion.

"I suppose it will be admitted, that if there be repulsion, the electrical atmosphere, like the common air (or the repulsive influence, supposing electricity to be a liquid), is most intense at the surface of the body with which it is in contact, and consequently that the power of repulsion is greater at the circumference of a Leyden jar than at the centre. If so, a ball suspended in the jar, both being similarly electrified, ought, according to the theory of repulsion, to be repelled in every direction horizontally from the sides of the jar; and if compelled to move towards one side, ought instantly, when left at liberty, to fly back to the centre of it, because the power of repulsion would in such case be exercised by the side to which the ball approached in an increased degree, and by that from which it receded in a diminished degree. This repulsion towards the centre being found not to happen, seems a convincing proof that there is no power of repulsion.

"The ball will in fact remain in any part of the jar while it is electrified in the same degree with the circumference, the small quantity of air in the centre being unable to exert a sufficient attraction on its electricity to draw it there." I have long contemplated repeating this experiment with a large coated cylinder of air, the coatings being insulated and the outer one charged positively by a powerful machine, so that the inner coating should at the same time assume the positive state with reference to the central air, but have not yet been able to attempt to do so.

"I would now refer to the Treatise on Electricity, published by the Society for the Diffusion of Useful Knowledge, ch. 5, p. 20, s. 75, et seq. In s. 77 it is said, with respect to a sphere, that '*the whole* of the fluid is in consequence of the repulsion

of its own particles which is every where directed from the centre outwards, accumulated in a thin stratum at the very surface of the sphere.' This is directly affirming that the whole of the fluid is at the surface, even the 'very surface' of the sphere and that being 'at the *surface*' it exerts its power of repulsion 'from the centre' and that on '*its own particles*.' It is affirming that a power of repulsion is exerted from a centre and then concluding therefrom that the very seat and vehicle of that power is thereby removed altogether as far as it can go from that centre. It is in effect to affirm that the thing supported is its own support. In s. 75, it is said that 'a metallic conductor in the form of a globe or cylinder contains just as much electricity when hollow as it does when solid.' Here there can be no centre to contain the electricity which should repel that to the surface which is there.

"From all the views I have taken of the subject I am irresistibly brought to the conclusion, that however intense the electricity may be at the surface of a solid conducting body it must be as much so all the way to the centre if there be repulsion.

"If attraction only, I can easily conceive the attraction of the atmosphere overcoming the attraction of the body and drawing all its surplus electricity to the surface.

"The experiments of Coulomb mentioned in s. 79, upon a conducting body having pits and depressions in its surface, also confirm my ideas. If there be repulsion, the bottoms of those pits and depressions (where Coulomb's delicate proof plane and torsion balance showed no signs of electricity) ought to have had at least as much fluid as the extreme surface being nearer the centre of the body.

"In order to repel the electricity from entering those pits and depressions, surely there ought to have been electricity of at least an equal intensity in those pits and depressions."

A film of electricity may be imparted to the lining of a Leyden phial of 40 or 50 or perhaps 100 times as many strata deep as to the prime conductor, with which it is in intimate communication.* How is it possible to explain this consistently with a power of repulsion? Is it not a self-evident refutation of that doctrine?

It seems very surprising to me how tenacious philosophers have been of the strange and hypothetical doctrine of repulsion; a doctrine which has never yet been satisfactorily established as existing in any one instance in nature. In every supposed

* I shall endeavour to give an explanation of this singular phenomenon in some future paper.

instance of repulsion there are quite as satisfactory reasons to be given for the phenomena upon the principle of attraction.

Newton has taught us a rule not to admit more causes in explaining natural phenomena than are necessary; and yet philosophers either assume a second electrical fluid or a new power, that of repulsion in one fluid, to cut a knot which is as easily untied by the one undeniable fluid, and its one undeniable power, attraction, for other bodies.

What is the reason for assuming repulsion of positive electricity? The recession of two positively electrified bodies from each other. If that be a sufficient reason why is it not equally cogent to say that two negatively electrified bodies recede from each other and therefore negative electricity (whatever it be, an entity or a non-entity) has a power of repulsion?

Those who cannot admit two fluids see well enough the conclusion cannot be sustained as to negative electricity, and ought therefore to resign at once the assertions of the repulsion of positively electrified bodies, seeing it stands on no better, nor indeed on any other, foundation.

They consequently ought to adopt the word recession (as it expresses the effect without implying any particular cause) in lieu of repulsion which assumes every thing without the shadow of support from either fact or argument.

The following propositions may perchance be found to be consistent with all electrical phenomena, particularly this of electrical recession. I shall endeavour to illustrate them so far as they relate to that point in a third paper accompanied by diagrams.

My present opinions are

1. "That there is one fluid only.
2. "That that fluid possesses homogeneous attraction.
3. "That it most commonly exists in the state of a liquid.
4. "That it exists in contact with all bodies.
5. "That it so exists by exerting an attractive influence on bodies.
6. "That the electricity in contact with one body exerts an attractive influence on the different matter of adjacent bodies.
7. "That bodies of different natures have different affinities for electricity.
8. "That when the quantity of electricity in a body and the bodies surrounding it is in proportion to their attractive influence, the electricity is inactive or in equilibrio.
9. "That when electricity is added to a body, increased attractive influence is exerted between the electricity of that body and the adjacent bodies themselves.

10. "That when electricity is so added to a body, the attraction of that body itself for the electricity of adjacent bodies is diminished.

11. "That when electricity is abstracted from a body, increased attractive influence is exerted between the body itself and the electricity of the adjacent bodies.

12. "That when electricity is so abstracted from a body, the total amount of attraction, before exercised by its electricity on the adjacent bodies, is diminished.

13. "That when the electricity of the adjacent bodies is (as in proposition 11) called upon to exert an increased attractive influence in one direction only, its ordinary attractive influence is lessened in other directions and for the body with which it coheres, and vice versâ

14. "The like of the adjacent body itself, or, that when an adjacent body is called upon (as in proposition 9) to exert an increased attractive influence in one direction only, its ordinary attractive influence is lessened in other directions and for the electricity cohering with it, and vice versâ.

15. That the equilibrium being destroyed by added electricity, every adjacent particle of matter acquires a tendency to part with its electricity to particles of matter more remote from the electrified body and to attract electricity from particles of matter nearer to that body.

16. "The converse if the equilibrium be destroyed by abstraction of electricity."

17. That one cause of conduction is the excess of the attraction of the electricity for the conducting body over its homogeneous attraction.

18. That one cause of non-conduction is the excess of the homogeneous attraction of electricity over its attraction for the non-conducting body.

---

## XXII. *Organic Chemistry. Report on a memoir by* M. Fremy, *relative to the modifications that tartaric and paratartaric acid experience from heat.**

MM. Robiquet, Pelouze, and myself have been appointed by the Academy to report upon M. Fremy's memoir, the object of which is the examination of the modifications experienced by tartaric and paratartaric acids when exposed to a great heat, although incapable of converting them into

* From the Compte Rendu Hebdomadair des Séances de l'Académie des Sciences. April 30, 1838. Translated by Mr. J. H. Lang.

products properly called pyrogenic. M. Braconnet had remarked that tartaric acid subjected to fusion changes its property: M. Fremy wished to prove, and discover the cause of this fact, and has arrived at some very interesting results; a just recompense for the long work to which he has devoted himself.

The nature of oyxgenic acids may be explained by two theories, which probably are both true, but which will each apply only to a certain number of these bodies. One of these theories, that which is almost universally admitted, consists of regarding them as distinct bodies, of real oxygenic acids, which unite with water or bases to form salts. The other takes no account of these acids when they are anhydrous: it considers them when they are hydrated, as hydracids, and looks upon their salts as bodies analogous to chlorures.

These two theories are opposed in the most direct manner concerning the nature of the tartaric acid; for one of them, that which considers the tartaric acid as an oxacid, is incompatible with the analysis of the anhydrous emetic, and if from the composition of this body we would regard the tartaric acid as an hydracid, we experience some difficulty at first starting to account for the results observed by M. Fremy. They are in fact much more easy to explain by regarding the tartaric acid as an oxacid.

We will now take a survey of these results.

M. Fremy discovered the body which, in generally admitted ideas, ought to bear the name of anhydrous tartaric acid.

He obtained it with the greatest facility, for it was only necessary to expose the tartaric acid to the action of heat in a vessel. The acid dissolves, loses some water, finishes by puffing itself up, and leaves a spongy mass which chiefly consists of anhydrous tartaric acid, so little soluble in water that it might be separated by the assistance of this solvent of the parts of the tartatic acid incompletely deprived of water.

0.467 of anhydrous tartaric acid prepared by M. Fremy, under the inspection of your reporter, gave when analysed 0.626 carbonic acid and 0.139 water, whence we have

| | | Calculation. | Experiment. |
|---|---|---|---|
| Carbon | 306 | 36.8 | 37.0 |
| Hydrogen | 25 | 3.0 | 3.3 |
| Oxygen | 500 | 60.2 | 59.7 |
| | 831 | 100.0 | 100.0 |

Thus the body described by M. Fremy, under the name of anhydrous tartaric acid, possesses the properties and composition that he assigns to it.

We know that besides the tartaric there is also another acid which has been successively called racemic and paratartaric, and which in fact possesses in its salts the exact composition of the tartaric acid. In all destructive reactions the tartaric and recemic acid act in the same manner, so that at present nothing can enlighten us on the differences which may exist between the two, with regard to their rational form.

M. Fremy is about to submit the paratartaric acid to the same treatment that produced him the anhydrous tartaric acid. We might hope that on this occasion, some difference would be manifested between these two bodies.

There has been nothing. The paratartaric acted as the tartaric acid and produced an analogous body which we were obliged to designate with the name of anhydrous paratartaric acid. This circumstance appeared, to your reporter, so much to merit attention that he thought it necessary to multiply the verifications, as far as concerns the compositions of the anhydrous paratartaric acid.

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| I. | 0.317 | gave | 0.092 | water and | 0.425 | carbonic acid | ; |
| II. | 0.500 | „ | 0.150 | „ „ | 0.670 | „ „ | ; |
| III. | 0.332 | „ | | „ „ | 0.447 | „ „ | ; |

which would produce in hundredths,

| | I. | II. | III. |
|---|---|---|---|
| Carbon . . | 37.0 . . | 37.0 . . | 37.2 |
| Hydrogen . | 3.2 . . | 3.3 . . | „ |
| Oxygen . . | 59.8 . . | 59.7 . . | „ |
| | 100.0 | 100.0 | „ |

These three analyses perfectly agreeing with those of M. Fremy, leave not the slightest doubt of the existence of an anhydrous paratartaric acid compounded exactly like the anhydrous tartaric acid itself.

Besides these two facts, remarkable in themselves and above all on account of the two acids that have furnished them, M. Fremy has discovered two others which, by their novelty, have strongly fixed the attention of chemists who labour at the development of organic chemistry.

In fact, it is not anhydrous tartaric acid that is produced immediately on the dissolution of the tartaric acid. Before passing to this state the common tartaric acid gives rise to two intermediate products, of great interest for theory. The first is tartralic, the second M. Fremy's tartrelic acid.

The tartralic acid is produced by tartaric acid which, instead of saturating two atoms of base, only saturated $\frac{3}{2}$ atoms.

The tartrelic acid is produced in its turn by tartaric acid which would only saturate a single atom of base.

So that from the most probable formula for the tartaric acid

$$C^{16}H^{8}O^{10}, 2H^{2}O \text{ or } C^{16}H^{12}O^{12}$$

we see that the three products on which it acts would be represented in the following manner, in their respective salts of lead:

$C^{16}H^{8}O^{10}, 2PbO$ . . Tartrate
$C^{16}H^{8}O^{10}, \frac{3}{2}PbO$ . . Tartralite
$C^{16}H^{8}O^{10}, PbO$ . . Tartrelate.

Thus M. Fremy was convinced that in proportion as the tartaric acid loses its water, it gives rise successively to bodies which combine with smaller quantities of base and which assume the state of salt, with quantities of base equivalent to the proportions of water they have preserved.

These modifications remind us of those which have been with so much truth assigned by M. Graham, as the cuases of the variations that phosphoric acid and the phosphates experience by the action of heat.

The author is convinced that the tartralic and tartrelic acids easily return to the state of tartaric acid.

Your reporter has proved the analysis of the tartralate of lead. The salt subjected to experiment was enclosed with tartrelate, which is with difficulty avoided; but it has given, besides, for the analysis of anhydrous acid, results conformably to those of M. Fremy.

Hence we have from the labour of M. Fremy, that tartaric acid may lose water, by passing through modifications analogous to those of phosphoric acid, until it arrives at the state of anhydrous tartaric acid. The paratartaric acid is in the same case.

We would have wished that the two anhydrous acids on which he has operated had been submitted to the action of the bromine or of chlorine; that some experiments of this kind had been tried equally on the hydrated tartaric and paratartaric acids; but M. Fremy may do this hereafter.

M. Fremy has indeed introduced a new view into the study of organic acids, and which belongs entirely to him. He appears at first sight to have solved the question regarding their nature, since by discovering the anhydrous tartaric acid he seemed to place out of doubt the real formula of this acid in the hydrated state: but with a little attention, we perceive these new results to be easily explained when we consider the tartaric acid as an hydracid.

In fact, in proportion as the tartaric acid loses water, it gives rise to products whose capacity for saturation constantly diminishes until it becomes nothing; for the anhydrous tartaric acid is no longer an acid; and between this product and the tartrelic acid there are formed also other substances whose capacity for saturation is less than that of the tartrelic acid itself, and whose study deserves all the attention of the author.

Hence we may consider tartaric acid and M. Fremy's new acids as so many distinct hydracids.

As for the anhydrous tartaric acid, it would be a product of decomposition, but not an acid by itself.

Be it as it may from these theoretical views, necessarily presented to prove that the researches of M. Fremy in no way destroy the results given by the emetic analysis, it is clear that his labour deserves all the approbation of the Academy, by the novelty of the facts, their importance, correctness, and the excellent spirit in which they have been discussed.

We have, therefore, the honour of proposing to the Academy, to decide that M. Fremy's memoir be printed in the collection of "*Savans étrangers.*"

The conclusions of this report are adopted.

---

## XXIII. *Organic Chemistry. On new products extracted from Salicine. By* M. Piria.*

M. Dumas presents to the Academy some products recently produced in his laboratory and under his own inspection, by M. Piria, a young Neapolitan chemist. These products will hereafter give place to the reading of a developed memoir.

I much regret, says M. Dumas, that the customs of the Academy do not permit M. Piria himself to present the observations that I am about to have the honour of submitting for him in his name. But the importance of the facts that he has observed will justify his desire of giving them a prompt publicity.

M. Piria, who is occupied in the study of salicine with the greatest care, having submitted this substance to the action of sulphuric acid and chromate of potassa, obtained from it, besides the ordinary acid which is produced in such a case, a new oily product, perfectly comparable to an essential oil, and too plentiful to be regarded as an accidental matter.

* From the Compte Rendu Hebdomadair des Séances de l'Académie des Sciences, April 30, 1838. Translated by Mr. J. H. Lang.

This oil subjected to analysis, presents exactly the same composition as the hydrated benzoic acid. It has the same density as this body in a state of vapour.

So far it is only a case of isomeria, of which we have observed so many in organic chemistry.

This isomeria goes on further; for if we form it of a combination of new oil, with the oxide of copper, we find that it is represented in this case by the same formula as the anhydrous benzoic acid which is met with in salts.

But as soon as the oil in question is submitted to the action of chlorure, a production of chlorhydric acid is obtained, and at the same time the formation of a product crystallized in beautiful colourless flakes.

Bromine acts in the same manner. In both cases there is a loss of two atoms of hydrogen, replaced by two atoms of chlorure or of bromine.

Hence we have

$C^{28}\,H^{12}\,O^{4}$ oil
$C^{28}\,H^{10}\,Ch^{2}$ compound chlorure
$C^{28}\,H^{10}\,Br^{2}$ compound bromine.

These results so clearly remind us of those which M. M. Liébig and Vôhler, obtained when acting on the oil of bitter almonds, that we are led to represent them in an analagous manner.

Hence the new oil becomes

$C^{28}\,H^{10}\,O^{4}$ salicyle, hypothetic root
$C^{28}\,H^{10}\,O^{4}\,H^{2}$ hydrure of salicyle oil
$C^{28}\,H^{10}\,O^{4}\,Ch^{2}$ chlorure of salicyle
$C^{28}\,H^{10}\,O^{4}\,Br^{2}$ bromure of salicyle.

Which fully confirms this view of the subject, viz., that the hydrure of salicyle combines with the baryte, and forms a compound which is represented by

$$C^{28}\,H^{12}\,O^{4}\,,\,Ba\,O,\,H^{2}\,O\,;$$

desiccated in a current of dry air at 160°, this body not only loses the atom of water it encloses, but also another atom of water, thus leaving a real salicylure of barium

$$C^{28}\,H^{10}\,O^{4}\,Ba.$$

The hydrure of salicyle combines in the same manner with potassium; thus producing a salt which crystallizes in beautiful and large flakes of a gold colour. The analysis of this product agrees with that of the preceding.

It equally unites with ammonia, &c.

If we subject the chlorure of salicyle to cold, by the potassium, the two bodies combine and form a real soluble salt, whose acids precipitate the chlorure untouched.

The substance of the facts observed by M. Piria is represented by so simple a supposition, that it appears to me worthy of some attention.

I have looked upon the benzoic as a body susceptible of being represented by a carbure of oxided hydrogen,

$$C^{28}H^{10}+O^2.$$

This carbure of hydrogen, uniting with two atoms of oxygen, would hence furnish a root, the benzoic.

It is the same carbure of hydrogen, which, uniting with four atoms of oxygen, would produce a new root, the salicyle.

$$C^{28}H^{10}+O^4.$$

It is difficult not to be tempted to compare the root $C^{28}H^{10}$ with the azote itself, and we should then have the following series:

| | |
|---|---|
| $Az^2$ .......... | $C^{28}H^{10}$ |
| $Az^2O^2$ ........ | $C^{28}H^{10}O^2$ |
| $Az^2O^3$ ........ | $C^{28}H^{10}O^2+O$ |
| $Az^2O^4$ ........ | $C^{28}H^{10}O^4$ |
| $Az^2O^5$ ........ | ,, |

For the remainder, see the results of the analysis of M. Piria.

### *Hydrure of Salicyle.*

Two analyses of this substance have furnished the following numbers:

| | | | |
|---|---|---|---|
| I. | $0^g$ 445 | hydrure of salicyle | |
| | 0,195 | water .......... | H= 4,8, |
| | 1,117 | carbonic acid.... | C=69,4. |
| II. | $0^g$ 474 | hydrure of salicyle | |
| | 0,209 | water .......... | H= 4,89, |
| | 1,185 | carbonic acid.... | C=69,11. |

By calculation we should obtain the same numbers:

| | | |
|---|---|---|
| $C^{28}$ ...... | 1071,2 ...... | 69,3 |
| $H^{12}$ ...... | 74,9 ...... | 4,8 |
| $O^4$ ...... | 400,0 | |
| | 1546,1 | |

The following are the data relative to the density of the vapour of this substance:

| | |
|---|---|
| Excess of weight of the balloon full of vapour over one full of air . . . . . . . . . | 0g 421 |
| Capacity of the balloon, c. cub. . . . . . | 233 |
| Temperature of the vapour . . . . . . | 230 |
| Barometer . . . . . . . . . . | 0m,764 |
| Atmospheric temperature . . . . . . | 13 |
| Air remaining in the balloon . . . . . | 0,0 |
| Density of the vapour . . . . . . . | 4.276 |

We should have the following numbers by calculation:

| | | | |
|---|---|---|---|
| 7 | volume steam carbon | ...... = | 2.9512 |
| 3 | „ hydrogen | .......... = | 0.2064 |
| 1 | „ oxygen | ........... = | 1.1026 |
| | Calculated density | .... = | 4.2602 |

This density, it may be perceived, agrees with that which M. Mitscherlick and myself found for benzoic acid.

*Anhydrous salicylure of copper.*

We have made the analysis of a basic salicylure of copper, to have a neat result with regard to the elementary composition of salicyle and have obtained the following numbers:

0,827 salicyle combined with copper
0,135 water.................... H = 4,5
0,877 carbonic acid ............ C =74,2

By calculating the composition of salicyle, we should have:

| | | |
|---|---|---|
| $C^{28}$ ...... | 1071.2 ...... | 74,7 |
| $H^{10}$ ...... | 62.4 ...... | 4,3 |
| $O^{4}$ ...... | 400.0 | |

*Salicylure of barium.*

This salt is easily obtained neutral and anhydrated, and deserves the preference for fixing the atomic weights of the salicyle.

1.237 salt dried at the ordinary temperature in vacuo, has experienced

0.110 loss, after having been dried in a current of dry air at 160°. This loss answers to 8,8 p 100, or to 2 atoms.

0,522 salt dried at the ordinary temperature in vacuo has given 0,292 sulphate of baryte.

By calculation we should have in these two cases:

| | | | | |
|---|---|---|---|---|
| | | $C^{28}$ ...... | 1071,2 | |
| | | $H^{10}$ ...... | 62,4 | |
| Exper. | | $O^3$ ...... | 300,0 | |
| Baryte.. | 36,7.. | BO ...... | 956,9 | 36,6 |
| Water.. | 8,8.. | $2H^2O$...... | 224,9 | 8,6 |
| | | | 2615,4 | |

We have tried the elementary analysis of this same salicylure of barium and we have, as in all these analogies, had less carbon than was necessary.

0,595 salt dried at 190° in a current of dry air,
0,934 carbonic acid.

| Exper. | | Calculation. | |
|---|---|---|---|
| C = 43,4 ...... | $C^{28}$ ...... | 1071,2 | 44,8 |
| | $H^{10}$ ...... | 62,4 | |
| | $O^4$ ...... | 400,0 | |
| | Ba ...... | 856,9 | |
| | | 2390,5 | |

*Chlorure of Salicyle.*

This product has been analysed with care and has given the following results:

I. 0,645 chlorure of salicyle
0,591 „ of silver

II. 0,456 chlorure of salicyle
0,133 water
0,892 carbonic acid

Whence we have the following numbers:

| | | | | |
|---|---|---|---|---|
| Carbon .... | 54,1 .... | $C^{28}$ .... | 1071,2 | 54,2 |
| Hydrogen .. | 3,2 .... | $H^{10}$ .... | 62,4 | 3,1 |
| | | $O^4$ .... | 400,0 | |
| Chlorure .. | 22,6 .... | $Ch^2$ .... | 442,6 | 22,4 |

*Bromure of Salycyle.*

We have made several analyses:

I. 0,582 bromure of salicyle
0,539 „ of silver

II. 0,400 bromure of salicyle
0,089 water
0,608 carbonic acid

Whence we obtain the following numbers:

| | | | | |
|---|---|---|---|---|
| C = 42,05 | ...... | $C^{28}$ ...... | 1071.2 | 42.6 |
| H = 2,47 | ...... | $H^{10}$ ...... | 62.4 | 2.4 |
| | | $O^{4}$ ...... | 400.0 | |
| Br = 38,88 | ...... | $Br^{2}$ ...... | 978.3 | 38.9 |
| | | | 2511.9 | |

M. Piria continues his experiments; their object is to put out of doubt the exact proportion of hydrogen contained in his products, for that is the only element on which he can have any correction to fear.

Admitting that these analyses are confirmed by new proofs, it remains to find the means of passing from the benzoic to the salicyle and reciprocally. This will necessarily be the object of numerous attempts on the part of this young chemist.

## XXIV. *Answer to Mr. Harper's Queries at page* 466, *of the Annals for June,* 1838. *By* J. P. Simon, M. D. *Member of the Royal College of Surgeons, London.*

Mr. Editor,

In answer to Mr. J. Harper's queries respecting some electrical theories, I beg to submit it as my opinion, that the outside coating of a Leyden jar becomes negatively charged with an equivalent charge to that of its inside coating, not because glass is a conductor or a non-conductor of the electric fluid, but simply by induction, as it is impossible to collect or accumulate either fluid (positive or negative) without inducing the presence of the other on the opposite side; and in the same manner the pith balls put in motion by a chain from the prime conductor of the machine communicating with an inverted tumbler inclosing the pith balls, are put in motion by the opposite fluid thus excited on its interior by induction; and if Mr. Harper will repeat his experiment, and try what kind of electricity will be in the inside of the tumbler (by means of Bennett's gold leaf electrometer and a stick of sealing wax) exciting the balls, he will find it charged with negative electricity, unless the chain emanates from the negative part of the machine. Under such circumstances he would find the outside of the tumbler charged with positive electricity, although glass be said to be an electric or non-conductor; it nevertheless receives and retains either fluid brought in contact with it in proportion to the dryness of it, and to the state of the atmosphere at the time of the experiment: and it is a curious fact that no matter how moist the interior of the glass

may be, provided the whole of the exterior be perfectly dry, and in a dry atmosphere, the opposite fluid excited by induction, in virtue of the presence of the other, will make no attempt to leave the surface; and in the same manner may be accounted, why a jar fully charged may be handled without communicating the slightest electric sensation, although we know that its outside coating possesses a charge of the opposite kind equal in strength to that of its interior.

The reason why the sparks do not make their appearance on the outside of the spotted jar whilst in communication with unspotted jars (or battery) in the act of charging is simply owing to the modified descent of the fluid into it, and the uncoated portions of its outside become sufficiently good conductors (owing to the moisture always present on glass) to conduct slowly and invisibly the superabundant fluid from every coated spot to coated spot, which is also favoured by its being placed in an atmosphere of one kind of electric effluvia brought there from the general reservoir (the earth); if, on the other hand, the jar be spotted on both sides, the sparks will of course appear on both sides but not near so quickly as it would, were it charged alone. The divided stream coming from the machine first charges the lower spots, which giving up their superabundant quantity to the upper ones, the sparks then appear suddenly and a corresponding discharge takes place on the outside coating. I may be labouring under some mistake, but, Mr. Editor, such are my views of the matter, and if you think they will be calculated to convey any kind of information to your numerous readers, no matter how trifling, you will perhaps insert this letter.

I remain, dear Sir,
Your very humble and obedient servant,
J. P. SIMON.

4, *Bernard Street, Southampton,*
*July* 3, 1838.

P. S. I hope, Mr. Editor, that I shall not be laughed at, for stating it as my opinion that we are more indebted to the CREATOR of *all things* than certain philosophers are disposed to admit, for the wonderful results of our humble philosophical researches, and that we are aided in them by nature's own work; for instance, I do not think that when we turn our machines that we excite and accumulate the two fluids, (viz: the positive and the negative, for I have always been for the theory of the two) only "*one*" is excited by our mechanical effort, and the other is instantly supplied to us, and without effort, from the surrounding atmosphere; the positive alone

(or *vitreous as the French call it*) is excited by the friction of the glass against the amalgamated cushion and the negative, or resinous, is spontaneously developed by induction and in equal proportion and that without variation; in the same manner, I am of opinion, that from the voltaic battery never but one fluid, viz: the positive is excited by the chemical action of the exciting liquid on the metals, the active principle, however, emanating from the zinc and the development of the negative influence is brought "*there*" as a natural consequence and in equal proportion, by one of the laws of nature, that the one cannot be excited without the other, but *only one* can we excite by our artificial means, and the other is afforded to us by nature's own work.

J. P. S.

---

## XXV. *Chemical Composition of Asses' Milk.*

From a memoir on the chemical composition of asses' milk, by E. Péligot, we learn that this milk differs from others in the considerable proportion of sugar of milk which it contains, and that to this we ought in all probability to attribute its medical properties.

The medium of 16 analyses given in 100 parts of asses' milk,

| | | | |
|---|---|---|---|
| Solid matter | 9·53 | Butter | 1·29 |
| Water | 90·47 | Sugar of milk | 6·29 |
| | | Caseum | 1·95 |
| | 100· | | |

The solid portion varied in different specimens from 7 to 11 per cent of the milk.

The quality as well as quantity of the milk was found to vary materially with the food. Of four different kinds of provender, beets were found to yield a milk the richest in solid matter—next a mixture of Luzerneand oats, then potatoes, then carrots. The selected quantities of milk were, with

| | |
|---|---|
| Beets . . | 1·500 k |
| Oats and Luzerne | 1·500 |
| Potatoes . . | 1·250 |
| Carrots . . | 1·000 |

Much depends also on *the time* at which the milk is drawn: thus, milk taken at 1 hour and a half, 6 hours, and 24 hours after the previous milking, gave:

| | after 1½ hours. | after 6 hours | after 24 hours. |
|---|---|---|---|
| Butter | 1·55 | 1·40 | 1·23 |
| Sugar of milk | 6·65 | 6·40 | 6·03 |
| Caseum | 3·46 | 1·55 | 1·01 |
| Solid matter | 11·66 | 9·35 | 8·57 |
| Water | 88·34 | 90·65 | 91·43 |
| | 100· | 100· | 100· |

As this last result is directly contrary to common opinion, the experiment was repeated after a lapse of six hours and of twelve hours from the previous milking, and found to coincide with the above.

But, on dividing the milk, obtained at the same milking, into three equal portions, they were found to contain,

| | | | |
|---|---|---|---|
| Butter | 0·96 | 1·02 | 1·52 |
| Sugar | 6·50 | 6·48 | 6·45 |
| Caseum | 1·76 | 1·95 | 2·95 |
| Solid matter | 9·22 | 10·45 | 10·94 |

Thus proving the correctness of the common opinion, that the strippings afford the richest milk.

M. Péligot proved that by giving to an ass thirty grains of ioduret of potassium for six days, the milk contained iodine at the end of the period, as was evident by the action of starch upon it, and of a chloride. Ten grammes of common salt per day was detected by the taste of the milk, and by analysis. A chloride given, five grains per day to an ass, and twelve grains to a goat, was not discoverable in the milk.

The milk of an ass, to whom thirty grammes of bi-carbonate of soda was given for six days, was very alkaline even when first taken from the udder. This experiment was repeated fifteen times, whereas the milk of the ass has ordinarily an acid reaction. G.

*Annales de Chemie et Phys.*

*Jour. Frank. Ins.*

## XXVI. *Report on* Professor Morse's *Electro-magnetic Telegraph.*

The Committee on Science and the Arts constituted by the Franklin Institute of the State of Pennsylvania, for the

promotion of the Mechanic Arts, to whom was referred for examination, an Electro-magnetic Telegraph invented by Professor F. B. Morse, of the City of New York, report,

That this instrument was exhibited to them in the Hall of the Institute, and every opportunity given by Mr. Morse and his associate Mr. Alfred Vail, to examine it carefully, and to judge of its operation; and they now present the following as the result of their observations.

The instrument may be briefly described as follows;

1st. There is a galvanic battery of sixty pairs of plates, seven by eight and a half inches each, arranged according to the very convenient plan devised by Professor Hare, and set in action by a solution of sulphate of copper.

2d. The poles of this battery can be connected, at pleasure, with a circuit of copper wire, which, in the experiments we witnessed, was ten miles in length. The greater part of the wire was wound round two cylinders, and the coils insulated from one another, by being covered with cotton thread.

3d. In the middle of this circuit of wire,—that is at what was considered virtually at a distance of five miles from the battery, was the *register*. In this there is an electro-magnet, made of a bar of soft iron bent in the form of a horse-shoe, and surrounded by coils of the wire which form the circuit. The *keeper* of this magnet is at the short arm of a bent lever, at the end of the longer arm of which is a fountain-pen.

When the keeper is drawn against the magnet, the pen comes in contact with a roll of paper wound around a cylinder, and makes a mark with ink upon this paper. While the telegraph is in operation, the cylinder which carries the paper, is made to revolve slowly upon its axis, by an apparatus like the kitchen jack, and is at the same time moved forward so that the pen if constantly in contact with the paper would describe a spiral or helix upon its surface.

4th. Near the battery, at one of the stations, there is an interruption in the circuit, the ends of the separated wire entering into two cups, near to each other, containing mercury. Now, if a small piece of bent wire be introduced, with an end in each cup, the circuit will be completed, the electro-magnet at the other station will be set in action, the keeper will be drawn against it, and the pen will make a mark upon the revolving paper. On the other hand, when the bent wire is removed from the cups, the circuit will be interrupted, the electro-magnet will instantly cease to act, the keeper will, by its weight, recede a small distance from the magnet, the other end of the lever will rise and lift the pen from the paper, and the marking will cease.

5th. The successive connexions and interruptions of the circuit, are executed by means of an ingenious contrivance for depressing the arch of copper wire into the cups of mercury, and raising it out of them. This apparatus could not be described intelligibly without a figure; but its action was simple, and very satisfactory.

6th. Two systems of signals were exhibited, one representing numbers, the other letters. The numbers consist of nothing more than dots made on the paper, with suitable spaces intervening. Thus ... .. ..... would represent 325, and may either indicate this number itself, or a word in a dictionary, prepared for the purpose, to which this number is attached. The alphabetical signals are made up of combinations of dots and of lines of different lengths.

There are several subsidiary parts of this telegraph which the committee have not thought it necessary to mention particularly. Among these is the use of a second electro-magnet at the register, to give warning by the ringing of a bell, and to set in motion the apparatus for turning the cylinder.

The operation of the telegraph, as exhibited to us, was very satisfactory. The power given to the magnet at the register, through a length of wire of ten miles, was abundantly sufficient for the movements required to mark the signals. The communication of this power was instantaneous. The time required to make the signals was as short, at least, as that necessary in the ordinary telegraphs. It appears to the committee, therefore, that the possibility of using telegraphs upon this plan, in actual practice, is not to be doubted; though difficulties may be anticipated which could not be tested by the trials made with the model.

One of these relates to the insulation and protection of the wires, which are to pass over many miles of distance, to form the circuits between the stations. Mr Morse has proposed several plans,—the last being to cover the wires with cotton thread, then varnish them thickly with gum-elastic, and enclose the whole in leaden tubes. More practical and economical means will probably be devised; but the fact is not to be concealed that any effectual plan must be very expensive.

Doubts have been raised as to the distance to which the electricity of an ordinary battery can be made efficient; but the committee think that no serious difficulty is to be anticipated as to this point. The experiment with the wire wound in a coil may not, indeed, be deemed conclusive; but one of the members of the committee assisted in an experiment in which a magnet was very sensibly effected by a

battery of a single pair through an insulated wire of $2\frac{3}{4}$ miles in length, of which the folds were four inches apart; and when a battery of ten pairs was used, water was freely decomposed. An experiment is said to have been made with success, on the Birmingham and Manchester railroad, through a circuit of thirty miles in length.

It may be proper to state that the idea of using electro-magnetism for telegraphic purposes has presented itself to several different individuals, and that it may be difficult to settle among them the question of originality.

The celebrated Gauss has a telegraph of this kind in actual operation, for communicating signals between the University at Gottingen and his magnetic observatory in its vicinity. Mr. Wheatstone, of London, has been for some time also engaged in experiments on an electro-magnetic telegraph. But the plan of Professor Morse is, so far as the committee are informed, entirely different from any of those devised by other individuals, all of which act by giving different *directions* to magnetic needles, and would therefore require several circuits of wires between all the stations.

In conclusion the committee beg to state their high gratification with the exhibition of Professor Morse's telegraph, and their hope that means may be given to him to subject it to the test of an actual experiment made between stations at a considerable distance from each other. The advantages which this telegraph would present, if successful, over every kind heretofore used, make it worthy of the patronage of the government. These are, that the stations may be at a distance asunder far exceeding that to which all other telegraphs are limited,—and that the signals may be given at night and in rains, snow, and fogs, when other telegraphs fail.

(By order of the Committee.) WILLIAM HAMILTON,
*Philadelphia,* *Actuary.*
*February* 8*th*, 1838.

*Ibid.*

---

## XXVII. *Discovery of Arsenic in a human body, taken up three years and a half after burial.* By Ossian Henry, *Chemical operator of the Royal Academy of Medicine.*

The body of a woman which had been buried at the town of Sens three years and a half, was taken up in consequence of renewed suspicion that she had been poisoned by arsenic. The exhumation was performed with due formality,

the head and limbs were removed and the trunk was encased, legally sealed, and sent to Paris for examination. On being opened the body was found to be in perfect preservation, which was ascribed to the dry sandy soil in which it had been buried. A slight odour of rotten wood, owing to pieces of the coffin which accompanied it, was the only perceptible effluvium, while the brown bistre colour of the skin gave it the appearance of a mummy. The vicera had become hardened or corneous, and were so condensed and confounded into a membranous leafy mass as to be scarcely distinguishable from each other. The liver was sufficiently distinct; it had a waxy consistence and a deep brown colour. After a due anatomical examination, the mass of vessels was taken out, freed from the brown sandy powder dispersed through it, and the trunk was returned to Sens to be replaced in the grave.

Although the death was imputed to an arsenical compound, nothing was omitted relative to the presence of other posions; but the result of this enquiry being completely negative, the attention of the operators was confined to the evidence of the presence of arsenic.

The detached mass was divided very carefully by a scalpel into fine shreds or strips;—these were boiled during an hour, in two successive portions of distilled water, each acidulated by half an ounce of very pure hydrochloric acid, in order to promote the solubility of the arsenite or arseniate of lime which the body might contain, in consequence of the reaction which had supervened during the long time in which it had been in the ground. The boiled mass was thrown upon a filter of clean linen, and the brown liquid thus obtained, was exposed to the air until the next day. It was then found to be covered with a pretty thick scum of solid fat which was very easily separated.

The clear, brownish, acid fluid, thus obtained, was partly neutralized by pure ammonia, and a current of very pure hydrosulphuric acid was directed through it and kept up for a long time. The gas very soon gave rise to an abundant production of brownish magma, which in the course of forty-eight hours, was completely precipitated. The clear fluid, which was easily decanted from it, gave by analysis nothing more than a small portion of salts of no importance, some phosphate of lime, and a peculiar brown animal matter.

The precipitate was collected upon filtering paper (previously purified by hydrochloric acid and distilled water) and washed with care. In it was to be found the arsenic if any existed, in the form of sulphuret. To clear the precipitate

of the brown matter, it was treated repeatedly with very dilute ammonia; but the whole of it being thus dissolved, the new liquid product, of a brown colour, was evaporated to dryness by a heat cautiously regulated. The residue was a dark brown, dry, friable substance, which, put upon charcoal, gave out an empyreumatic, animal odour, which was followed by the smell of garlic, decidedly manifest.

The dry substance being separated into two equal parts, one of them, A, was triturated with alcoholised potash and carefully dried until it became pulverulent. Mixed with black flux and exposed to the blow pipe in a narrow tube, thick, fuliginous, empyreumatic vapours were given off, and a volatile product gathered in the narrow part of the tube, forming a shining metallic ring of a steel grey, and which proved to be metallic arsenic.

The other part of the brown product was mixed with pure nitrate of potassa and strongly calcined in a new porcelain capsule, until the white residue was entirely deprived of animal matter. This was dissolved in distilled water and exactly neutralized by very pure nitric acid. The test of nitrate of silver, then threw down a very abundant *precipitate of a brick red colour*, consisting of *arceniate of silver.*

This salt, washed and dried, produced with the blow pipe and black flux as in the former case, a reflective ring of metallic arsenic. The product of these two processes amounted to seven or eight grains of the metal.

That nothing might be neglected to prove the existence of the poison, we placed a quantity of it in a long open tube, and holding it slightly inclined over a lamp, the metal was speedily oxidised by contact with the air, and yielded a white lameller crystallization. This was boiled in distilled water and the solution was precipitated.

1. Yellow,—by nitrate of silver.
2. Green,—by ammoniacal sulphate of copper.
3. In Yellow flocculi, soluble in ammonia, by sulphuretted hydrogen.

All doubt is thus removed of the existence of arsenic in this body, and that it was to this substance that the death of the individual must be ascribed. In publishing the account, we do not pretend to exhibit a peculiar case, but from the very considerable portion of the poison which we were able to separate, it was thought there might be some benefit in making it known. The example confirms those previously established, and it may afford consolation to humanity, by proving that criminals are not sure of impunity because the earth has for a long time concealed their victims.

*Jour. Pharmacie*, 1837.

*Note by the Translator.*—The results of the analysis described in the foregoing paper, are perfectly satisfactory as it respects the evidence of the existence of arsenic. Had the experimenter been aware of the method of detection by means of the simple and elegant little apparatus, described by James Marsh (Vide Jour. Frank. Inst. Vol, XVIII, p. 338.) they might easily have given an additional proof of the presence of the metal. This method removes also one source of doubt which sometimes arises from the carbonaceous matter contained in the flux, which may of itself produce a ring in the tube which has much the appearance of a metallic surface, though it consists only of shining charcoal. The very small quantity of material, which this little instrument will operate upon, as well of metal which it will detect, is another strong recommendation to its adoption. We would propose, in the use of it, the substitution of a small plate of clear mica in lieu of the glass, which, as the author admits, often breaks with the heat.

*Ibid.*

---

## XXVIII. *On the various uses of Steatite.*

Steatite is a kind of soapy marl, or talc, sometimes white, at others green, or grey, and more rarely red or yellow. Its sp. grav. is from 2.60 to 2.66. It is composed of silex, alumine, magnesia, oxide of iron and water, but it varies in different localities. It is very common in Germany, in Cornwall, and it exists also in the western part of France. As it requires a very high heat for fusion, and is cut or wrought with great facility, very good crucibles can be made of it, which fire hardens and litharge penetrates very easily. It is employed for moulds in metallic castings. It is used in England in the manufactory of porcelain. M. Vilcot of Leige, has made a great number of trials to ascertain whether this substance can be employed by Lapidaries; he has made Cameos of it, to which he has given a fine brilliancy by heat, and such a degree of hardness as to give sparks, with steel. He has succeeded in colouring them yellow, grey, and milk white, by the addition of various solutions. By polishing them on stone, he has given them all the splendour of agate, and has obtained some pieces which resembled onyx, but the fire very soon obliterates the veins, which cannot be reproduced. Having a great affinity for glass, steatite, reduced to a very fine powder, answers very well when mixed with other colours, for painting on glass. It is used also as a kind of sympathetic pencil for writing on glass, leaving no traces when the glass

is rubbed with woollen cloth, but becoming again visible by breathing freely upon it, and disappearing again as the glass becomes dry. Workers and embroiderers of silk prefer it to chalk for tracing, because it is more durable and does not affect the colours of the stuff. As steatite has the property of uniting with oils and fatty substances, it enters principally into the composition of the balls used for cleaning silk and woollen stuffs. It is the basis also of some pigments. It gives a fine brilliancy to marble, to serpentine and gypseous stones. Mixed with oil, it is used to polish glass and metallic mirrors. If newly prepared leather be powdered with it and allowed to dry, it gives it, when rubbed with horn, a very fine lustre. Steatite is employed for glazing paper, by being spread over it in very fine powder, or better by being mixed with the colouring matter, and then glazing by rubbing with a brush. The powder of steatite, from its unctuosity is one of the substances which give the easiest play to vices and screws, and diminish friction in wheels. Mixed with tallow it furnishes a very favourable material for preserving machinery.

Steatite is easily cut with a saw, turned in a lathe, and smoothed with a plane. It may, therefore, be worked into any shape, and afterwards if necessary be rendered very hard. When the artist has finished his design he places it in a covered crucible, surrounds the crucible with charcoal in a furnace, raises the heat gradually, keeps it for two or three hours in nearly a white heat and allows it to cool slowly. When it comes out, it is so hard as to strike fire with steel and to blunt the best files.

White specimens of steatite acquire a milk white by exposure to heat; those which are coloured assume a grey or reddish tint, but they may be variously coloured by the aid of oily, alcoholic, acid, or alkaline solutions. Colours that dissolve in amber varnish, such as verdigris, ochre, &c., colour steatite, when heated by charcoal. Colours dissolved in spirits of turpentine are the most lively. Solutions of carthamus (saffron flower) gamboge, campeachy wood, dragons blood, &c., in spirits of wine, colour steatite by steeping it in them several hours. Solution of gold in aqua regia, gives a purple colour, of a shade depending on its strength. Muriate of silver colours it black when aided by sulphuric acid. Sulphate of indigo—a greyish blue. If steatites, coloured by nitro-muriate of gold, or muriate of silver, be exposed to a bright flame, it assumes the metallic colour of gold or silver.

When the stone is heated, colours dissolved in acids are rapidly and finely attached to it, and hence a cameo ground, of any particular colour, is easily obtained. Sulphuric acid

produces more effect than nitric and muriatic. Oxalic acid may be successfully employed. Colours dissolved by alkalies, and especially indigo, serve to colour steatite. In general these colours penetrate about one eighth of a line into the body of the stone. We are indebted to M. Moy for these researches.

When the stone is baked, it is polished, as usual, with emery, tripoli, or tin putty. It acquires much brilliancy, resembling agate, jasper, calcedony, &c. It is easy from these facts to infer, that the engraver may avail himself of this substance, on account of its softness, since he may perform upon it in one day as much as he could do upon hard stones in a week; and when it has passed through the fire, his work acquires a hardness and durability almost unlimited.

*Jour. des Conn. Usuelles*.

*Ibid.*

---

XXIX. *On the Decomposition of Carbonate of Lime by Heat.* *By* M. Gay Lussac.

It was long ago asserted that the calcination of calcareous rocks was favoured by the presence of water; and this opinion appears to have been admitted even by the greater number of those who were habitually employed in the preparation of lime. M. Dermas admits the influence of water as unquestionable and gives this double explanation of the fact, that it (the water) either acts upon the carbonate by forming an ephemeral hydrate, expelling the carbonic acid and taking its place until it is itself expelled by a red heat; or, that the water, being decomposed by the carbon employed as fuel various gases are produced, one of which is carburetted hydrogen. This, reacting upon the carbonic acid of the carbonate, tends to convert it into carbonic oxide, and thus facilitates its separation from the carbonate of lime. Hence, limestone recently extracted and consequently still moist, must be more easily calcined than stone that is nearly dry. The greater number of limeburners are acquainted with this fact, and therefore they water the stone which has been long taken from the quarry, before they place it in the kilns.*

But the first of these two modes of explanation is inadmissible, since hydrate of lime is decomposed at a heat considerably below that which is essential to the decomposition of carbonate of lime under the influence of the vapour of water.

* Dumas. Traité de Chimie, &c., II., 481.

The second explanation, considering the circumstances of the combustion of lime kilns, does not appear to me to apply to the facts. I shall not, therefore, stop to examine it, but proceed to state the observations, which, if I am not mistaken, will furnish the true explanation of the influence of water in the calcination of calcareous rocks.

Having filled a porcelain tube with fragments of marble, I placed it over a furnace whose temperature was easily regulated. To one end of the tube was adjusted a glass retort containing water, and to the other end a glass tube for conveying the carbonic acid. The heat was first raised to the point necessary for decomposing the marble; and then, by accurately closing the ash-hole it fell to a dark red, and the carbonic acid ceased to be given out. At this juncture the water in the retort was made to boil; then the carbonic acid reappeared in abundance. When the vapour was intercepted the flow of gas instantly stopped and was restored only by readmitting the steam. The circumstances were thus successively varied, and with them the results themselves.

It appears thus demonstrable, that the vapour of water really favours the decomposition of carbonate of lime by heat, and that by its concurrence this decomposition takes place at a temperature inferior to that which is ordinarily required.

The action of the water appears to me purely mechanical. When carbonate of lime, exposed to heat, begins to undergo decomposition, there is formed around it an atmosphere of carbonic acid, which presses on the acid still in combination; so that in order to become disengaged it must overcome the pressure of this atmosphere. Now this can take place only by a previous elevation of temperature,—or, by removing the carbonic acid and leaving a vacuum,—or, otherwise by displacing it either by the vapour of water or some other elastic fluid, such as atmospheric air, &c.

This explanation is justified by the following experiment. I brought some carbonate of lime, in a porcelain tube, to a temperature a little inferior to that at which it began to decompose, and then I caused a current of atmospheric air to pass into the tube. The disengagement of carbonic acid immediately recommenced and continued in connexion with the current of common air,—stopped when it stopped, and was renewed whenever it was readmitted.

It appears then to me, evident, that the influence of watery vapour, in the burning of lime is limited to the production, merely, of a vacuum for the carbonic acid, and thus preventing a pressure upon that which remains associated with the lime. With the presence of vapour a less elevated tempera-

ture is necessary to dislodge it; but still we must not overrate the importance of this vapour. The water in calcareous rocks is mechanically interposed between their particles; and, if we except a trifling portion which remains confined in the centre of pieces so large that the heat does not reach and evaporate it, a large portion of the water must evaporate without producing any useful effect, but on the contrary, with a loss of fuel, before the stone has acquired the temperature needful to its decomposition.

I am convinced then that the vapour of water favours the calcination of limestone, but I am doubtful of the real advantage which it is supposed to produce, because there is no great difference between the temperature at which carbonate of lime is decomposed alone, and that at which it is effected with the concurrence of water. Besides, if the steam exerts only a mechanical action similar to that of atmospheric air, we see no important advantage it can yield over the aeriform current which incessantly penetrates the calcareous mass subjected to calcination.

The increased facility of the decomposition of limestone by aqueous vapour, or more properly, by a vacuum, ought not to remain an isolated fact. It may be regarded as an established principle, that when decomposition by heat, or by a chemical agent, must necessarily produce one or more gaseous elements, it may in general be favoured by retaining the substance in a vacuum, or by preventing the elastic fluids disengaged from pressing on it. And, reciprocally, that we may retard or entirely prevent decomposition by forming round the body the pressure of an elastic fluid, of the same nature as the one disengaged. It was thus, in the curious experiment of Hall, that carbonate of lime was brought into a state of fusion at a very high temperature, without undergoing decomposition, and the influence of an adequate pressure of carbonic acid.

*Ann. de Chimie et de Phys. Oct.* 1836.

---

## XXX. *London Electrical Society.*

*Tuesday, 3d July.* A Communication from Mr. Nayler of Southsea, was read; the subject of which was, the local attraction in iron built steam vessels on the magnetic needle. The local attraction in ordinary built vessels, and the mode adapted for its correction were described; and the author considers that the same laws in respect to the attraction of the magnetic needle must follow in iron built as in other vessels, although in an increased degree. If this law were to be once accurately

ascertained all danger would cease. Our limits will not permit us to enter fully into the mode proposed by the author, except that he forcibly points out the necessity of the local attraction being accurately examined previous to the departure of the vessel, as also at the different ports at which she may touch during her voyage.

A paper was read by Mr. Maugham, Lecturer on Chemistry to the Royal Gallery of Practical Science, on a new compound of hydrogen and carbon, formed by the action of the voltaic battery on charcoal points whilst under water. When the points are held in distilled water at a sufficient distance to prevent ignition, ordinary electrolytical action will proceed; the water will be decomposed, hydrogen and oxygen gases being evolved at the electrodes; but when an approximation of the charcoal points takes place and the brilliant light so well known to experimentalists is perceived, *carbonic oxide gas* escapes which may be collected in a tube in the ordinary way.

The peculiar compound which was the main subject of Mr. Maugham's paper, is also formed by keeping the charcoal points in a state of voltaic ignition under water; and is collected in a tube, or other receiver, properly placed for its reception. This compound Mr. Maugham calls *protohydruret of carbon*. (See "Annals of Electricity, Vol. I, page 250.") The *protohydruret of carbon* is a gaseous body, perfectly transparent and colourless; is possessed of a peculiar fetid smell, nearly resembling that of sulphuretted hydrogen gas; and from the disagreeable taste which it communicates to the water under which it is generated, one would imagine it to be absorbable to a considerable extent, by that liquid.

Mr. Maugham also alluded to an experiment which he had made some years ago, viz: that by dividing the pieces of charcoal into several points the light would be proportionably increased, each point giving out an equal intense flame.

The facts which were alluded to in Mr. Maugham's paper were mostly illustrated experimentally by that gentleman by means of a voltaic battery of about a hundred pairs of Wollaston's double four inch plates; and other suitable apparatus. Carbonic oxide was formed from the ignited charcoal points, and afterwards transferred to an eudiometer. To this gas a proper proportion of oxygen, (liberated from water by the voltaic battery) was added. The gaseous mixture was then exploded by an electric spark, and carbonic acid gas was the result; as was demonstrated by its well known action on lime-water.

Mr. Maugham's entire paper shall be placed before our readers as soon as it is disposed of by the Electrical Society.

Mr. Sturgeon exhibited a contrivance for multiplying the discharges of an electro-magnetic coil so as to cause more frequent sparks and shocks. This instrument is described in the Annals, page 32 of the present volume.

*Tuesday, 17th July.* The secretary read a paper written by C. Griffin, Esq. of Leamington, entitled *An enquiry into the cause of electrified bodies receding from one another.* This paper is inserted in the present number of the Annals. (See page 36.)

Previous to the chair being taken, Mr. Coombes, of New York, exhibited to the members a locomotive engine propelled by electro-magnetism. This machine, which is at present exhibiting at the Royal Gallery of Practical Science, consists—1st. Of the carriage containing the apparatus, but which, in consequence of the arrangements necessary to the taking out of a patent in this country not being completed, could not be explained. 2d. Two voltaic batteries; these are made on the principle of Hare's calorimoter, excited with a strong solution of sulphate of copper. 3d. Two carriages attached to the apparatus. The apparatus, batteries, and carriages weigh 60 or 70 lbs., and are placed on a circular rail-road. Upon connexion of the apparatus being completed (by means of four cups containing mercury) with the poles of the two batteries, the apparatus is set in motion and revolves for some time with considerable velocity, but which decreases as the action of the battery diminishes.

Mr. Coombes afterwards read a short paper consisting principally of extracts from American periodicals as to the probable effect which the use of such machines may have in superseding the use of steam.

## *Remarks.*

Mr. Coombes's paper excited a good deal of interest amongst those who were present at the meeting; and brought on a very animated discussion. The paper claimed for Mr. Davenport a good deal of credit on certain points which some of the members of the Society thought were not due *solely* to him. Mr. Coombes had said in his paper that Mr. Davenport was the first person to apply Electro-magnetism as a motive power for propelling mechanical arrangements; whereas it was well known that he had been anticipated in several countries. Dr. Henry's plan for giving a reciprocating motion to an electro-magnetic bar; and the contrivances of M. Jacobi,

Mr. Mc.Gauly, and others, had all been described in the scientific journals before anything was known of Mr. Davenport's machine; or even of his experiments. With regard to *rotatory* electro-magnetic engines, the probability was, that the first of them was exhibited in London, as long ago as March, 1833. Much credit, however, was due to Mr. Davenport for his ingenuity and perseverance. Much praise was also given to Mr. Ransom Cook, for the facilities and support he had so handsomely given to Mr. Davenport in bringing forward his inventions. We only want a few such liberal and spirited capitalists, in this country, to bring to light many a bright gem which now lies smothering under the oppressive loads of adversity and neglect.

EDIT.

## XXXI. REVIEWS AND NOTICES OF NEW BOOKS.

*The fifth Annual Report of the Royal Polytechnic Society.* SIMPKIN and MARSHALL, *Stationers' Court: and* WEAL, *High Holborn.*

We have read this little volume with much pleasure and great interest. It is one of those delightful pictures of mental progression, and displays of scientific skill and ingenuity which are the inestimable fruits of those sources of incitement, those fertile Associations of intelligence, so peculiar to our own times. Happily, we have many Societies and Institutions in this Country, whose influence, in their respective localities, have been productive of much useful intelligence and scientific knowledge; but we are not aware that any of them have been supported with that degree of spirit and liberality which we have observed in the Royal Cornwall Polytechnic Society: nor does it appear that any other has made such rapid progress as this truly valuable Association.

The volume before us contains, besides an account of the general business of the Society for the current year, 1. The laws and regulations of the Society. 2. The Committee's "Report," which occupies sixteen closely printed pages of excellently composed matter: comprising a general outline of the progress of physical knowledge in that important district; of the present condition of the Society; of the distribution of the premiums; of their influence in exciting competition; and of the general objects and prospects of the Society. Speaking of the prizes for "Statistical Essays," and of the information already obtained by that means; of the "degrada-

tion of the poorer classes, in the metropolis and many of our large towns;" the report emphatically concludes in the spirit of genuine philanthropy. "It shall be no unworthy object of your Society to take a part, however humble, in furthering investigations which help to solve problems of deep, social, and economical importance; which instruct and interest us in the physical and moral condition of our fellow-men: which point out the fulcrum whereon to rest the lever, in our endeavours to remove the mass of sin and misery, which depress so large a portion of our species; and which thus enables us to associate, more immediately, scientific and intellectual pursuits, with works of benevolence and the rich luxury of doing good." 3. *An Essay on the diseases peculiar to Miners. By* Mr. RICHARD LANYON. This production, which occupies twenty-six pages, displays much talent, and an extensive knowledge, both general and local, of the circumstances connected with the subject. 4. Description of "Jones's Machinery for raising Miners:" illustrated with a copperplate. 5. "Capt. Tregaskis's Spring Hammer." 6. "Trevelyan's Life Boat." 7. Trevelyan's Printing Press." 8. Renolds's Instrument for drawing Scrolls." 9. "Daniel's improved bottle Jack." 10. "Lamination of Clay." By Robert Were Fox, Esq. 11. "Observations on the Duty and Horse-power." By J. S. Enys, Esq. 12. "Meteorological Register." By Mr. L. Squire." 13. "A statement of the *greatest* and *least* quantities of Water in *Imperial Gallons*, discharged per minute from the Mines in 1827; specifying the months in which it reached its maximum and minimum." *Extracted from Captain T. Lean's Report of Steam Engine Duty, &c. for December,* 1837.

---

*On the causes of Planetary Motion. With a diagram. By* JABEZ ALLIES, ESQ. WILLIAM EDWARDS, 12, *Ave Maria Lane.*

Nine pages, out of the twelve which constitute this pamphlet, are occupied with an attempt to show that the cause of planetary motion is electricity. The other three pages occupy the appendix, which, as it is but short, we offer to the notice of our readers.

EXPERIMENT.

"Cut a couple of cards each into a circle of about two inches in diameter; perforate one of these at the centre, and fix it on the top of a tube, say a common quill; make the

other card ever so little concave, and place it over the first, the orifice of the tube being thus directly under and almost in contact with the upper card. Try to blow off the upper card,—you will find it impossible."*

SOLUTION.

"The reason why the upper card cannot be blown off, is because the current of air which passes through the two cards, from the blowing through the tube, is in a more rarefied state than the surrounding atmosphere, and therefore the latter presses upon the two cards so long as the blast is kept up, and causes them to adhere together. This adhesion may be proved, either by fastening a bit of thread upon the top of the upper card, and when in the act of blowing very strong, lift it perpendicularly up, and the adhesion will be perceived; or, it will appear more clearly by holding the cards upside down, and when the blast is strong, take the hand away from under the concave card, and it will adhere to the other, although downwards, as long as the blast continues. The stronger the current of air is, the stronger is the adhesion; and when the cards are upside down, they will adhere together the same, whether the breath is exhaled or inhaled between them.

"The author also tried the experiment by blowing with a pair of bellows affixed to the quill, and this produced exactly the same effect.

"From the above it clearly appears, that air, in a state of agitation or currents, whether it be cold, as from the bellows, or warm, as from the mouth, is in a more rarefied state than the surrounding atmosphere."

## XXXII. MISCELLANEOUS ARTICLES.

### *Lamination of Clay by Electricity.*†

Some clay was exhibited by R. W. Fox, Esq. which had become laminated by long continued voltaic action, so as to resemble clay-slate in its structure.

Fig. 44, Plate IV. may serve to illustrate the process by which this was accomplished. Let *a*, *b*, *c*, *d*, represent the top or rim of an earthenware cup or basin;—*e*, a piece of

* The above experiment was published in the year 1833, in some of the journals, and the author of this pamphlet then wrote the accompanying solution.

† From the fifth Annual Report of the Royal Cornwall Polytechnic Society, 1837.

copper pyrites;—*f*, the upper edge of a plate of zinc;—*i*, copper wire by which the two latter were connected;—and *g*, *h*, the top of a mass or wall of clay between the copper ore and zinc; and forming for each of them, a water-tight cell. The cell containing the copper ore was filled with a metallic solution;—the sulphate of zinc, for instance, and the other with water mixed with a little sulphuric acid.—The water with which the clay was worked up, was also acidulated. Thus circumstanced, the apparatus was set aside for three or four months, and was not disturbed till some little time after the water had evaporated, and the clay had become perfectly dry throughout.

It then exhibited, on breaking off a portion of its upper part, lines of cleavage of a schistose character, parallel to the sides of the clay and plate of zinc, or, at least, as nearly so as was consistent with their undulatory form. In other words, the lines or laminæ, were at right angles to the direction of the electrical forces.

They are indicated by the lines on *g*, *h*; and the strongly marked line, *a*. *c*, represents a principal line of division which separated the clay into two portions, from the top to the bottom.

These seemed to form, as it were, two voltaic plates, in opposite states of electricity, and one of them, consequently more favourable than the other for the reception of metallic deposits, and other bases from their solutions. These results, which, however, were much more marked in some experiments than in others, seem to confirm the correctness of the views stated in the last Polytechnic Report, pp 87, 88.

Indeed, the general laminated structure of the clay appears to indicate, that a series of voltaic poles were produced throughout the clay, the symmetrical arrangement of which had a corresponding effect on the structure of the clay. This view is still more strikingly confirmed by the occurrence, in several instances, of veins, or rather laminæ of oxide of iron, the edges of which are shown by the shaded lines, *k*, *l*, *m*. In these cases sulphate of iron was substituted for the sulphate of zinc;—and laminæ of oxide of copper were sometimes formed, in like manner, when a solution of that metal was employed: and moreover, numerous minute insulated portions or specks of the oxide of copper were detected in different parts of the mass of clay when broken.*

* Some of these effects were apparently most decided when a series of voltaic exciters were used so as to increase the energy of the action;—three or four such cups, for instance, as shown in fig. 44, arranged so as to form a complete circuit.

The coloured laminæ produced in some of these experiments, have, apparently, a direct bearing on the variously coloured and waving lines, which the edges of the laminæ of schistose rocks so commonly present, and also, on the occasional deposition of metalliferous substances parallel to the cleavage, which seem to have been formed on or before the consolidation of the rocks. The true veins, however, were evidently formed after the consolidation of the latter; and as they generally intersect, in Cornwall at least, the laminæ of the "killas" or clay slate, at more or less considerable angles, they seem to have acted the part of the copper wire, shown at *i*, in completing the voltaic circuit.

Hence arises the question whether the productiveness of lodes, depends more or less on their bearings with respect to those of the cleavage; and it appears to be a matter of practical importance to ascertain these relations in our mining districts, and especially to note the angles at which the laminæ are intersected in their horizontal bearing and underlie, by lodes which yield given ores in the greatest abundance.

---

## *Speculations respecting Electro Magnetic Propelling Machinery.*

*By the Editor of the Journal of the Franklin Institute.*

In our number for November last, Vol. XX, p. 340, we published the specification of Mr. Davenport's patent for a machine intended to furnish a motive power by the agency of Electro Magnetism, to which we appended some remarks upon the subject generally. We had hoped, ere now, to have received more definitive information than has transpired, respecting the progress of the experiments which are being made in New York with a view to its testing the utility by applying it to drive a Napier Press, requiring a two horse power; we have hitherto learnt nothing of the result, of this proposed experiment; and suppose, therefore, that the trial has not yet been made.

Since publishing the article above alluded to, it has appeared to us that should a much less power be attained by such a machine than that which is now sought for, say the power of a man only, it would still be equally valuable with the steam engine, and would produce as great, if not a greater change, in the economy of the useful arts, as has been produced by that instrument; this, however, is under the proviso that the

cost of materials consumed in performing the work of a day should be less than that given for the labour of a man. Who is there who would not under such circumstances, need such a machine?

If we hire a man by the day we must not allow him to be idle; as in that case we give our money for nothing. The current of his life flows on, and he must be fed and clothed or the stream will stop. But give us a machine which is not costly at first, and if it works but one hour in the twenty-four, will itself be a consumer in that proportion only; a machine which we can at any moment set to turn our lathes, our grindstones, our washing machines, our churns, our circular saws, and a catalogue of other things which it would be no easy task to make out; such a machine would also perform a million of other operations by the conversion of a rotary into a reciprocating motion; and we again ask who is there among us who would not want one? Our farmers, our mechanics, and our housekeepers generally, must also be supplied. We could no more submit to live without it, after it has been once introduced, than we can now submit to travel at the slow rate of ten miles an hour, an event which we have learnt to think one of the miseries of human life.

With such a machine at our command we should soon wonder how we could have lived so long without it; and if taken from us it would leave a most awful chasm in the necessaries of life, of the existence of which our fathers never dreamed, and which happily we could not be called upon to witness so long as the store house of nature would enable us to obtain zinc and sulphuric acid at a cheap rate.

The steam engine cannot be used to advantage where it has not the labour of several horses to perform, as, whether large or small, it requires the constant attention of the engineer, or of the fireman, and is kept at work at an expense which is relatively increased as its power is diminished. One giving the power of a man only would be employed, at a cost which would pay the hire of two or three men, and if used but for an hour or two in the day, the expense would be incalculably increased; of course it is not, and never will be used, under such circumstances.

Let it not be said that we are prophesying about what is to happen; not so by any means; but be it remembered that we are speaking of what is a possible contingency. We have no doubt respecting the practicability of obtaining the power of a man by the agency of electro magnetism; we believe that such a machine may be kept at work without any considerable tax upon the time of the person using it, and we further

believe that the only thing which can prevent its coming into use is, the cost of the materials employed in operating it; the statements which we have heard upon this point are extremely contradictory, and upon the whole, are far from encouraging; the time, however, is not remote when this point will be determined.

*Jour. Frank. Ins.*

---

*On an Easy Method of preparing Platina Black.—By* M. Doboreiner. *Translated by* John Griscom.

Melt platina ore (crude platina) with double its weight of zinc, and treat the alloy thus obtained and reduced to powder,—first, by dilute sulphuric acid, and then by dilute nitric acid, in order to oxidate all the zinc. This, contrary to theory, is but slowly effected, even at a high temperature. A dark grey insoluble powder of native platina remains, mingled with osmiuret of oridium. This power acts like platina black, after it has been properly purified by lixivium of potash, and it has such an oxidising action that it transforms not only formic acid into carbonic, and alcohol into acetic acid, but even the osmium which it contains absorbs oxygen from the air, and is disengaged by degrees in the state of osmic acid.

This method of preparing platina in an extremely divided state, was recommended by Descotils thirty years ago, and he was the first to observe that the powder thus obtained detonates by heat like gunpowder, and that muriatic acid destroys this property.

When platina powder prepared by zinc is moistened with alcohol, it becomes incandescent and osmic acid is disengaged; but if it be mixed with alcohol so as to form a paste, and spread out on a watch glass, it disengages only acetic acid. This is the most simple process of purifying the air of a chamber. *Jour. des Mines.*

---

A process of purification rather too troublesome and expensive when ventilation and lime can be easily resorted to. It may, perhaps, be eligible in a sick room.

*Ibid.* Tr.

## *Prospectus of the Lectures in which* MR. STURGEON'S *Electro-Magnetic Engine was exhibited.*

# WESTERN

# Literary and Scientific Institution.

PATRON—THE KING.

1833.

SYLLABUS OF

## THREE LECTURES

ON

**Electricity, Magnetism, Electro and Thermo-Magnetism, and Magnetic Electricity.**

BY WILLIAM STURGEON, ESQ.

### LECTURE I. THURSDAY, MARCH 7.

### ELECTRICITY.

Introduction—Electric Fluid a distinct species of Matter—Its Natural available Sources, the Earth and the Atmosphere—Its conspicuous Natural Phenomena: Lightning, Aurora Borealis, Meteoric Star, Whirlwind, Waterspout, Camsin—Its Artificial Sources: Electric Machine, Atmospherico-Electro Vannus, Electrophorus, Electric Column, Voltaic Battery, Electric Kite, Thermo-Electric Combinations—Influence of Electricity on Vegetation, on Chemical and Magnetic Phenomena—Properties of Electric Matter available in experiment.

### LECTURE II. THURSDAY, MARCH 14.

### MAGNETISM.

Magnetic Iron Ore, or Native Loadstone—Transient Magnetism of Soft Iron—Permanent Steel Magnets—Influence of Magnetism on Watches and Chronometers—Phenomena of the Compass and Dipping Needle, illustrated by the Magnetarium—Magnetic Phenomena, produced by Atmospheric Electricity—Effects of Lightning on the Magnetic Needle—Local Magnetic Attraction on board of Ship—The manner of using Mr. Barlow's correcting plate, shown by a beautiful model of a man-of-war.

### LECTURE III. THURSDAY, MARCH 21.

### ELECTRO & THERMO-MAGNETISM & MAGNETIC ELECTRICITY.

Oersted's Experiments—Magnetic Phenomena produced by Electric and Galvanic Machines and by Electro and Thermo-Combinations—Theory of Terrestrial Electro-Magnetism—Ferro-Electro-Magnetic Sphere, for illustrating Terrestrial Electro-Magnetism—Unequal distribution of Electric Matter on the metallic body—Magnetic Electricity—Electric Spark produced by magnetic excitation—Ferro-Electro-Magnet rotated on its axis by the same Electric Currents that give it Magnetic Polarity—An Engine, whose motive power is Electro-Magnetism, will be exhibited turning Mill-work—Conclusion.

THOMAS SNELSON, *Secretary.*

THE ANNALS

OF

*ELECTRICITY, MAGNETISM,*
*AND CHEMISTRY;*

AND

Guardian of Experimental Science.

*OCTOBER*, 1838.

# XXXIII. *Further Observations on Voltaic Combinations. In a Letter addressed to* Michael Faraday, *Esq. D.C.L. F.R.S., Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal & Imp. Acadd. of Sciences, Paris, Petersburgh, &c. By* J. Frederic Daniell, *F.R.S., Prof. Chem. in King's College, London, &c.**

Received March 30,—Read April 6, 1837.

My dear Faraday,

I had intended, ere this, to have addressed you upon the subject of the *measure of affinity* which the *constant battery* may be made to supply, as indicated by the concluding experiment of my last letter; but my attention has been diverted, and the whole of my leisure occupied by what I found to be a necessary preliminary investigation of the effects of changes of temperature upon the voltaic action. In the course of my experiments upon this principal subject, I have also been led to observe some curious disturbances and diversions of the battery current, from secondary combinations; and I now submit the results of the whole inquiry to your judgment, not without a hope that you may consider them of sufficient interest and importance to be communicated to the Royal Society.

You may perhaps recollect that the standard charge, which I finally adopted in the use of the constant battery, was a mixture of eight parts of water with one of oil of vitriol on the side of the zinc, and a saturated solution of sulphate of copper in contact with the copper; and that the average amount of its work, as measured by the voltameter, was 11 cubic inches of

* From the Philosophical Transactions, Part 1, for 1837.

mixed gases per five minutes. It occurred to me that the resistance to the current might again be reduced by dissolving the salt in the standard acid instead of water; and upon making the experiment I found the action increased from 11 cubic inches to 13 cubic inches, at which rate it steadily maintained itself; the following being the result of one series of observations.

| Time. | | Interval. | Voltameter. |
|---|---|---|---|
| h | m | ′ | Cubic inches. |
| 11 | 0 | | |
| 11 | 5 | 5 | 13 |
| 11 | 10 | 5 | 26 |
| 11 | 15 | 5 | 39 |
| 11 | 20 | 5 | 52 |

Upon one occasion I prepared the charge by adding one part of oil of vitriol to eight parts of the saturated solution of the sulphate, and poured it into the cells whilst of the high temperature produced by the disengagement of heat during the mixture, which was about 110°; and the following series of experiments will show the great increase of action which followed this accession of heat, and its rapid decline with the temperature. The observations were made at intervals of two minutes; but I have added the calculated rate per five minutes to facilitate comparison with former experiments.

| Time. | | Interval. | Voltameter. | 5 min. rate. |
|---|---|---|---|---|
| h | m | ′ | Cubic inches. | Cubic inches. |
| 12 | 52 | | | |
| 12 | 54 | 2 | 8·8 | 22· |
| 12 | 59 | | | |
| 12 | 61 | 2 | 8· | 20· |
| 1 | 3 | | | |
| 1 | 5 | 2 | 7·3 | 18·2 |
| 1 | 7 | | | |
| 1 | 9 | 2 | 7· | 17·5 |
| 1 | 11 | | | |
| 1 | 13 | 2 | 6·5 | 16·2 |
| 1 | 14 | | | |
| 1 | 16 | 2 | 6· | 15· |
| 1 | 17 | | | |
| 1 | 19 | 2 | 5·7 | 14·2 |
| 1 | 21 | 2 | 4·8 | 12· |

Wishing to follow up this indication of the influence of temperature, I caused a tub to be made which would just receive the ten cells of the battery standing upon small blocks of wood, between which there was room for the syphon tubes

to pass. In this situation it was fresh charged with the acid solution; and henceforth I carefully noted the temperature, which in this instance was 68°. Its action was steady for three quarters of an hour at 13·8 cubic inches per five minutes. Two fluid ounces of fresh standard acid were then poured into the inner cells, and the tub was filled with hot water of the temperature of 130° to nearly the top of the cells; the action was now found to be 20 cubic inches per five minutes. The temperature of the exterior solution of sulphate was 106°, and of the interior acid 100°. The experiment was terminated by the bursting of all the membranes which had been exposed for five weeks to the acid solution.

Having remounted the battery, I proceeded to ascertain the effect of different charges in connexion with this decided influence of temperature; and, in the first place, having poured a plain aqueous solution of sulphate of copper into the exterior cells, I filled the interior with pure water, making use at the same time of fresh amalgamated zinc rods. At the commencement of the experiment the temperature was 74°; and there was no appreciable decomposition in the voltameter. The tub was then filled with water at 120°. When the temperature of the interior cell had reached 100° a slow action commenced, which became steady at 0·6 cubic inches per five minutes. The voltameter itself being now also heated to 115°, the following results were obtained.

| Time. | | Interval. | Voltameter. | 5 min. rate. |
|---|---|---|---|---|
| h | m | ′ | Cubic inches. | Cubic inches. |
| 11 | 21 | | | |
| 11 | 26 | 5 | 0·8 | 0·8 |
| 11 | 31 | 10 | 1·6 | 0·8 |
| 11 | 51 | 30 | 5·0 | 0·8 |

At the expiration of the experiment the rods were found to be only slightly tarnished, and no copper had penetrated to the interior cells.

The next charge which I made trial of was a solution of muriate of ammonia, in the proportion of two pounds of the salt to a gallon of water for the interior cells, and aqueous solution of sulphate of copper for the exterior. The results were as follow:

| | per 5 min. |
|---|---|
| ° | Cubic inches. |
| At 74 out of water . . . . . . | 5·5 |
| At 94 in water . . . . . . . . . . | 8·8 |
| At 124 in water . . . . . . . . . | 12·5 |

Upon this occasion I first observed that a portion of the

current was discharged by the water in which the battery wa immersed.

The battery plunged in water of the atmospheric temperature of 74° had been working steadily for twenty minutes at the rate of 3·1 cubic inches per five minutes: when the water was drawn off from the tub the rate immediately rose, and was maintained at 4·2 cubic inches per five minutes.

That a discharge may take place from the copper of one cell to the copper of the next, when the regular circuit is interrupted between the two, I had many opportunities of observing in the powerful currents with which I had been experimenting; for I have frequently seen it pass in the form of a spark when the cells had been too much approximated in the air; and when in water it was indicated by the frothing between the two from the disengagement of gas. In such a case there is no doubt that one of the zinc rods is thrown out of action, and the copper of that cell merely acts as an electrode to the antecedent zinc. I shall hereafter point out to you how readily a portion of a current may be diverted from its principal course to by-paths, if I may so express myself, which may be open to it.

The decomposition of the secondary electrolytes, and the course of the ions in this combination, are worthy of some remark. In several instances when the battery had been in action for a considerable period, the zinc rods were found thickly studded with beautiful, large, transparent crystals of sulphate of zinc. The solution of this salt was abundantly precipitated by nitrate of baryta and by potassa, the precipitate in the latter case being redissolved by an excess of the alkali. It was not in the slightest degree affected by nitrate of silver; proving that no muriate or chloride existed in it. There were no indications of free ammonia in the exterior cell: the precipitated copper however did not exhibit the beautiful bright pink hue which it ordinarily presents, but was of a dull, greyish, earthy appearance, resembling that of copper over which ammoniacal gas has been passed at a red heat, and probably contained some combined nitrogen. I had not, however, time to examine a product which is worthy of further investigation.

I tried one more experiment with a view to complete the inquiry into the probable advantage of changing the battery charge. I placed a saturated solution of common salt in contact with the zinc, and filled the exterior division of the cells with a saturated aqueous solution of sulphate of copper. With a temperature of 70° the following series of observations was made.

| Time. | | Interval. | Voltameter. | 5 min. rate. |
|---|---|---|---|---|
| h | m | ′ | Cubic inches. | Cubic inches. |
| 11 | 11 | | | |
| 11 | 15 | 4 | 3·5 | 4·3 |
| 11 | 19 | 8 | 5·9 | 3· |
| 11 | 23 | 12 | 8·0 | 2·6 |
| 11 | 27 | 16 | 9·9 | 2·3 |
| 11 | 31 | 20 | 11.6 | 2·1 |
| 11 | 34 | | | |
| 11 | 54 | 20 | 4· | 1·0 |

A fluid ounce of muriatic acid was added to each interior cell, and the action was only brought up to 2·7 cubic inches per five minutes. It thus appeared that the substitution of solutions of the muriates for dilute sulphuric acid, was in every way disadvantageous; and it was moreover found that when the circuit was broken the copper became seriously injured by their action and the formation of a submuriate of that metal.

Wishing now to extend the inquiry into the influence of high temperatures upon the voltaic current, and finding that the membranous tubes would not be able to resist the action of the acid under these circumstances, I endeavoured to find some substitute for the partition of the cells which would not be liable to injury from this cause. After several trials I found that porous earthenware, of the same texture as that of which wine coolers are commonly made, would answer my purpose sufficiently; and the arrangement which I ultimately adopted simplifies the construction of the battery to such a degree as to render it advantageous even under circumstances of ordinary temperature.

The interior cells consist of tubes of this earthenware, closed at the lower end, of the diameter of 1½ inches, and of the same height as the copper cells. The bottoms of the latter are fitted with sockets, in which the tubes are placed, and which confine them in their proper positions; the perforated copper plates, or colanders, for the reception of the solid sulphate of copper, pass over their upper ends. The tubes can be easily removed and instantly replaced; and the facility of emptying and refilling them renders the addition of syphon tubes unnecessary, except in very particular circumstances. Previously to their use they require to be thoroughly soaked in dilute acid; and I have not found them liable to injury, provided the sulphate of zinc be not allowed to crystallize within them; in which case they become disintegrated from the expansive force of crystallization. When the battery is out of action they may always be removed and emptied, and preserved in a state for immediate use by immersing them

in very dilute acid. Liquid conduction is not carried on quite so perfectly through their substance as through the membranes, on account of the less perfect communication between the liquids on their opposite sides; but when the acid sulphate of copper is used the amount of action at ordinary temperatures is from 7 to 8 cubic inches per five minutes, which is quite sufficient for all ordinary purposes, and is moreover perfectly constant and steady.

Having thus prepared the battery, I caused a circular steam vessel to be made of tin plate, round which the cells could be placed upon blocks of wood, and closed in with a cover, in which there was a socket which could at pleasure be connected with the steam pipe of a boiler. Two other sockets were also conveniently placed, which were stopped with corks, through which the electrodes of the battery could pass, when the proper connexions were made. I intended to have experimented with a series of ten cells, but owing to a mistake only nine could be conveniently arranged in the steamer.

The general result of numerous experiments was, that, the working rate of the battery having been ascertained at the ordinary atmospheric temperature, when steam was admitted it immediately began to rise; and at the full temperature of 212° was more than doubled, provided no *seondary action* interfered with it. The following series will be sufficient to illustrate the progress of the current: the temperatures were taken by a thermometer immersed in the steam.

| Time. | | Interval. | Voltameter. | 5 min. rate. | Temperature. |
|---|---|---|---|---|---|
| h | m | ′ | Cubic inches. | Cubic inches. | ° |
| 10 | 0 | | | | |
| 10 | 5 | 5 | 7·5 | 7·5 | 58 |
| 10 | 10 | 10 | 15· | 7·5 | 58 |
| | | | Steam admitted | . . | 110 |
| 10 | 25 | | | | |
| 10 | 30 | 5 | 9· | 9· | 170 |
| 10 | 35 | 10 | 20·5 | 11·5 | 195 |
| 10 | 40 | 15 | 37· | 16·5 | 200 |
| 10 | 41 | | | | |
| 10 | 46 | 5 | 19· | 19· | 205 |
| 10 | 51 | 10 | 39· | 20· | 206 |

Wishing to ascertain whether any portion of this increased effect were owing to a simple increase of conducting power in the electrolyte, or whether it were wholly dependent upon the electro-motive force, or increased energy of affinity, I set the battery in action with a voltameter included in the circuit, the body of which was immersed in water, which could be con-

veniently raised to the boiling temperature by means of a lamp. The observations were commenced at the temperature of 58°.

| Time. h m | Interval. ′ | Voltameter. Cubic inches. | 5 min. rate. Cubic inches. |
|---|---|---|---|
| 11 25 | | | |
| 11 30 | 5 | 6·5 | 6·5 |
| 11 35 | 10 | 13· | 6.5 |
| Voltameter heated to 130°. | | | |
| 11 46 | | | |
| 11 51 | 5 | 6·5 | 6·5 |
| Voltameter heated rapidly to 212°. | | | |
| 11 56 | 10 | 14· | 7·5 |
| 12 1 | 15 | 21·5 | 7·5 |
| 12 6 | 20 | 29· | 7·5 |

The battery itself was then heated by steam to 135°.

| Time. h m | Interval. ′ | Voltameter. Cubic inches. | 5 min. rate. Cubic inches. |
|---|---|---|---|
| 2 10 | | | |
| 2 15 | 5 | 13 | 13 |

A cell was then included in the circuit charged with cold standard acid, and without any sulphate of copper.

| Time. h m | Interval. ′ | Voltameter. Cubic inches. | 5 min. rate. Cubic inches. |
|---|---|---|---|
| 2 20 | | | |
| 2 25 | 5 | 8 | 8 |
| Cold cell removed. | | | |
| 2 30 | 10 | 20 | 12 |
| Cold cell restored with acid sulphate of copper. | | | |
| 2 35 | 15 | 29·5 | 9·5 |

The temperature of the battery was then increased to 160°.

| Time. h m | Interval. ′ | Voltameter. Cubic inches. | 5 min. rate. Cubic inches. |
|---|---|---|---|
| 2 38 | | | |
| 2 43 | 5 | 11 | 11 |
| 2 47 | 4 | 20·5 | 14 |

A small portion only of the increased effect would thus appear to depend upon the simple conducting power, which alone could have any influence in the voltameter with platinum electrodes; whilst the increased energy of the electro-motive force was checked by including in the circuit a cell whose

cells, with their connecting wires, will be represented by the nine smaller circles 1, 2, 3, 4, 5, 6, 7, 8, 9. In the experiment which I am about to describe, the electrodes *e* and *f* were unconnected, but all the short connecting wires were in their places. Each cell was tested by the galvanometer by means of extra wires, as at *g*. The cells 1, 2, 3, 4, 5, 6, 7, separately deflected the needle in the normal direction, or from 30° to 40° E. No. 8 deflected it in the opposite direction, or 40° W. The connexion between 8 and 9 was broken, and the same cell deflected the needle 35° E. The connexion was then broken between 8 and 7, and the deflection was 30° E.; and when both wires were replaced, the needle returned to 40° W.

The whole circuit was then completed by the connexion of 1 and 9 by a short wire, when, notwithstanding a path was open for the circulation of the battery current, the deflection caused by the single cell No. 8. increased to 55° W., which was contrary to the direction of the main circulation. While the circuit was thus complete, the other cells were again tested, with the following result:—

No. 8.—55° W.
7.—45 E.
6.—35 W.
5.—10 E.

With No. 4. the needle oscillated from one side of the coil to the other in the most extraordinary manner, first striking with considerable force against the pin on one side and then upon the other. Sometimes it seemed to hesitate between the two, and then to recede in one direction and advance in the opposite by sudden starts and jumps. These oscillations lasted for more than an hour, during which the experiment was continued, with equal force. The other cells were found,

No. 3.—10° W.
2.—20 E.

There was a strong spark upon breaking the connexions of all these secondary currents, but they would not pass through the voltameter.

These experiments were frequently repeated with the same general results; sometimes a cell in one position indicating a reverse current, sometimes one in another, the same cell occasionally passing to the normal direction, and at times oscillating violently between the two.

To determine whether the metal steam vessel had any influence upon these currents, five of the cells were removed to a table, and connected in series with a galvanometer, the

needle of which was permanently deflected 90° E. When, in addition to the galvanometer connexion, the two extreme cells were connected by a similar wire to those between the other cells, the needle was still deflected 15° E.: so that notwithstanding a shorter path was open to the battery current, and that through a conductor of considerably greater substance than the wires of the galvanometer, a portion still passed by the longer path. Under these circumstances, No. 1. cell was tested by a separate galvanometer, which was deflected 40° W. When the extra short connexion of the battery was broken, the deflection from No. 1. fell to 30° W.; and when this cell was totally unconnected with the others, its current was in the normal direction, or 50° E. When the short connexions were all restored, it again returned to 40° W. I must here observe, that the galvanometers, not being of the same construction, were not used as accurate measures of the force, but only to indicate the direction of the currents, and occasionally to show that the force was increasing or declining. The other cells were tested in the same way, with the following results:—

| | | |
|---|---|---|
| No. 2. | Battery connected by short wire . . | 30° E. |
| | Short connexion broken . . . | 55 E. |
| No. 3. | Battery connected . . . . . . . | 30 W. |
| | but changed to E. | |
| | Unconnected . . . . . . . . | 55 E. |
| No. 4. | Battery connected . . . . . . . | 20 E. |
| | Unconnected . . . . . . . . | 50 E. |
| | Again connected . . . . . . . | 40 W. |
| | Again unconnected . . . . . | 50 E. |
| | Again connected . . . . . . . | 40 W. |
| | changed to 30° E. | |
| | and swang violently E. & W. | |
| No. 5. | Battery connected . . . . . . . | 35 E. |
| | Unconnected . . . . . . . . | 55 E. |
| | Connected . oscillated E. & W. | 30 |

Hence it appears that this variable current may be produced from the single cells of the battery, under ordinary circumstances, when the whole series is connected by short wires.

I was still desirous of ascertaining whether these currents would be produced in the simplest possible arrangement, viz. when the elements consisted of amalgamated zinc, copper, and dilute sulphuric acid. For this purpose the five cells were thoroughly cleaned, and charged entirely with standard acid, without sulphate of copper. They were connected in series with a voltameter, and each cell was separately tested by the galvanometer, with the following results:

The extra connexion was then entirely removed, and

The deviation of No. 1. was 80° E.
No. 2. — 45 E.

Now with regard to the main battery current, when the extra connexion was wholly removed, the whole passed through No. 1. galvanometer with a certain resistance, and was measured by its deflection 80°: when the long wire was added, a portion was diverted into the new channel, and was measured by the decline of the needle to 70°. As the length of the extra wire was shortened, the resistance of this passage decreased; and more and more of the current was diverted from the galvanometer till the deflection of the needle only amounted to 15°, and nearly the whole passed through the extra wire.

The effect of these varying resistances upon the secondary current I think I can explain with the help of the annexed diagram. Let the circles 1, 2, 3, 4, 5, (fig. 59.) represent sections of the five battery cells, and the lines between 3 and 4, 4 and 5, 5 and 1, and 1 and 2, with the arrow heads, the short wires between the zinc of each cell and the copper of the next, with the direction of that conventional current which is supposed to flow from one to the other. Let A B C represent the long wire of a galvanometer, or the electrodes of a voltameter through which the circuit is completed between 2 and 3, and by which the current is resisted. When a secondary connexion is formed by the wire *a b c d*, between the zinc *e* and the copper *g* of the cell No. 1, a portion of the main current, which tends to pass through the electrolyte to the copper at *f*, being obstructed in this direction, passes to *g*, and completes its circuit through the wire *a b c d*, and the *diverted current* obviously will flow in the same direction as the main current, or from the copper through the wire to the zinc.

But if, instead of a resisting communication the primary circuit be completed between 2 and 3 by a short wire, as between the other cells, and as represented at fig. 60, and a secondary circuit be formed as before with the cell No. 1. by the wire *a b c d e*, a portion of the main current flowing from the copper of No. 5. to the zinc of No. 1, instead of passing through the electrolyte from *g* to *h*, finds a passage through the wire *a b c d e* to *h*, and consequently will appear to flow in an opposite direction to the primary current or from the zinc to the copper. The resistance of the circuits may be so adjusted that the current may sometimes take one course and sometimes the opposite, and produce those oscillations of the needle from E. to W. which have been just described.

The breaking of the secondary circuit did not affect the galvanometer of the primary current, but the breaking of the primary circuit always turned the secondary current into the normal direction, and increased the deviation of its needle; it reduced it, in fact, to the condition of the direct current from the single cell.

It is however obvious that these *diverted* currents of the complete circuit would not of themselves be sufficient to account for the stoppage of the main battery current of which we are in search; nor has it yet been shown how they were produced when the main circuit was broken, as represented at fig. 58. In search of the explanation of these phenomena I turned my attention to the influence of the metallic steam-vessel upon the voltaic arrangement.

When the battery was connected in the usual way with a galvanometer, and the needle was deflected 80° E., if the zinc electrode were lifted and made to touch the tin case in any part, it would remain deflected in the same direction 30°. If, on the other hand, the main circuit were broken at the copper electrode, and it were brought into contact with the tin, the deflection would be to the same amount in the same direction. The difference of that connexion is shown at fig. 61, where *t z* represents the zinc electrode in connexion with the tin, and passing to the zinc cup of the galvanometer G and the copper electrode C C in its usual position. On the other hand, *z' z'* represents the zinc electrode in its usual communication with the galvanometer, and the copper electrode *c' t'* in connexion with the tin. The most striking result of this experiment is, that notwithstanding the connexion of the tin with the galvanometer is reversed, the current is in the same direction; or that the current, which we must conceive to flow from the tin in one connexion, must flow to the tin in the other. To simplify the conditions of the experiment, I repeated it with a single cell. At the atmospheric temperature of 52°, when the circuit was completed in the usual way through the galvanometer, the deflection was 57° E.; but no deflection was obtained by making connexion with the tin. When steam was admitted, the ordinary battery current increased to 65°, and then contact with the hot tin produced a deflection of 20° E. by either electrode.

Four cells were now placed in the steam case upon plates of glass, and the action of the battery with the usual connexions being first tried at the temperature of 54°, was found as follows:

| | |
|---|---|
| Galvanometer alone . . . . . . . | 70° E. |
| Galvanometer with voltameter . . . | 50° E. |
| Voltameter alone . . . . | 5 cubic inches per 5 minutes. |
| Voltameter with galvanometer | 4 cubic inches per 5 minutes. |

At this temperature there was no current from the tin by either connexion.

When the steam was turned on, the battery current increased rapidly to 85° E. by the galvanometer, and there was a current from the tin connexions notwithstanding the glass insulation. This *extra current* was also found to exist during the flow of the main battery current; and when the two were measured by separate galvanometers,

| | |
|---|---|
| The battery current was . . . | 85° E. |
| And the extra current . . . | 30° E. |

It again made no difference whether the tin took the place of the zinc or the copper in the arrangement; the deflection was always in the same direction.

The breaking of neither current affected the other.

To ascertain whether heat alone, independent of the steam, was the cause of this extra current, a tin plate was placed upon the hot sand of a sand-bath, and the battery cells transferred to glass plates upon it. At first no extra current was detected; but as the temperature rose to 150° and 200°, the second galvanometer was affected to the amount of 50° or 60°; and this whether the battery circuit was complete or not. A deflection was even occasioned by making the contact with any part of the iron stove, however distant, with which the tin plate was in contact.

These experiments were repeated and varied numberless times, but not with uniform results: sometimes the extra current had sufficient intensity to pass through a voltameter, producing slow decomposition of the water; but most frequently, however great the deflection of the needle, it would not pass through this obstacle. At other times, in apparently similar circumstances, the extra current could not be detected at all. Whenever produced, however, it was always observed to flow from the tin to the battery, whether the connexion with the latter were made with the zinc or the copper.

I now placed one of the battery cells upon a piece of wood in an iron case made to receive it, of the same height, but having a space all round it of about an inch. When the primary circuit was completed by means of a galvanometer, the deflection was 60° E.; but there was no action upon a second galvanometer included in a secondary circuit between the iron and the zinc or the iron, and the copper of the cell. A little dilute sulphuric acid was then poured into the iron case, and immediately a strong extra current was produced. Under these circumstances

| | |
|---|---|
| The primary current was .................... | 60° E. |
| The extra current copper and iron connexion ... | 20 E. |
| Zinc and iron ............................. | 40 E. |

The analysis of the phenomena of these extra currents was most satisfactorily performed in the following manner:—in figs. 62, 63, and 64, let *i i i i* represent the section of the iron case, *c c c c* the section of the copper cell, and *z z* the zinc rod: let G also represent the situation of the secondary galvanometer with its different connexions with the circuit. In fig. 62, the connexion with the iron is made with the copper cup of the galvanometer, and the zinc cup is connected with the copper of the cell; and we see at once that a current is established, which, setting out from the iron, passes through the electrolyte to the copper, and completes its circuit through the galvanometer in the direction 1, 2, 3, 4, 5, 6, 7 8, 9. In fig. 63, the connexion with the iron remaining the same, the battery cell is connected by its zinc rod with the zinc cup of the galvanometer, and we have a powerful reversed current, which we must suppose to set out from the zinc, and to pass through a portion of the electrolyte to the copper, and from the copper through another portion of electrolyte to the iron, and to complete its course in the direction 1, 2, 3, 4, 5, 6, 7, 8, 9. In fig. 64. the main battery circuit is likewise completed, and the primary current will flow in the direction I., II., III., IV.; while the extra circuit, although apparently connected, as in fig. 63, with the zinc, is in fact connected with the copper, as in fig. 62, by means of the main battery connexion, and will convey the extra current from the iron through the electrolyte to the copper, and from the copper through the galvanometer back again to the iron in the direction 1, 2, 3, 4, 5, 6, 7, 8, 9.

It will be seen that the two currents coincide in their direction in that part of their circuits which is common to both, viz. III., IV., and 3, 4.

There is no difficulty at all in understanding how an extra current is established from the iron to the copper in addition to the main current from the zinc to the copper; but I was for a long time puzzled to make out how an extra current could pass from the zinc through the electrolyte to the copper, and from the copper through the electrolyte to the iron: it seemed to me that the interposed copper must act as a retarding plate, upon the opposite surfaces of which hydrogen and oxygen must be evolved; and that the intensity of a single circle could not be sufficient to force this passage. You will, I dare say, remember suggesting an experiment which led to the explanation of the difficulty.

Some dilute sulphuric acid was poured into a basin, and a platinum crucible containing some solution of sulphate of copper was placed in it. An amalgamated zinc rod wrapped

in filtering paper moistened with dilute acid, to prevent it from precipitating the copper by its local action, was held in the crucible. A plate of iron was also immersed in the acid in the basin; contact with the platinum being carefully avoided. A metallic communication was then made by means of a wire with the zinc and the iron, and we had thus the exact circumstances of the battery cell in the iron case, except that platinum was substituted for copper. No current, however, was thus formed, and no copper was precipitated upon the platinum from the solution of sulphate. A piece of copper plate was now placed under the platinum crucible, and in contact with it: the current immediately passed, and copper was precipitated upon the interior surface of the crucible from the sulphate. The current of the single circle could not pass by the retarding plate of platina, when oxygen must have been evolved on one side and hydrogen on the other; but when the oxygen was absorbed by the copper, and the hydrogen by the oxide of copper, these concurring affinities enabled the current affinity to make good its circuit.

To vary the experiment with regard to the metallic part of the combination, three of the battery cells were placed upon zinc plates with interposed flannel moistened with dilute sulphuric acid; the copper was thus placed between two generating plates of the same metal. When the three were connected together in regular series with a galvanometer, the deflection of the needle was 90° E. When a secondary connexion was made from the zinc rod of each cell in succession through another galvanometer with the zinc plate on which it stood, the deflection occasioned by the extra current was 20° W. In this case it must have flowed from the zinc through the electrolyte in the flannel to the copper; from the copper through the electrolyte in the cell to the zinc rod; and from the zinc rod through the wire back to the zinc plate. When the main circuit was broken the extra current changed its direction, and occasioned a deflection of the needle 20° E. Upon restoring the primary current the extra current again returned to its original direction, and so invariably upon many successive repetitions of the experiment.

When the extra connexion was made between the copper of the cell and the zinc plate, the deflection of the second galvanometer was always 40° W., or opposite to the main current, and was not disturbed by any interruption of the latter.

Being now satisfied that these extra currents were hydro-electric, and dependent upon the action of a liquid upon the metal which was brought into association with the regular voltaic combination, I examined more carefully the circum-

stances of the arrangement in which I had supposed that I had insulated the cells, and cut them off from any such influence by placing them upon thick glass plates. I now ascertained that the establishment of the extra current was owing to a thin film of moisture formed upon the glass, either by the condensation of steam, or slight leakage from the cells. At ordinary temperatures no action was thus excited, but when the temperature of the combination was sufficiently exalted very energetic currents were sometimes developed by a quantity of moisture, which might well have escaped ordinary observation. When great care was taken to make the glass plates perfectly bright and dry, the extra current was never produced.

It was now also clear that not only could independent extra currents be established, but that different portions of the main battery current could be diverted into this secondary path, and thus the occasional decomposition of water in the voltameter by the extra current could be accounted for.

There is one more relation of the *battery current*, the *diverted current* and the *extra current*, which it may be worth while to point out when they are all three established at the same moment. Let 1, 2, 3, fig. 65, represent the section of three battery cells, all standing upon blocks of wood in iron cases, the sections of which are represented by A, B, C, the bottoms of the cases being covered with dilute acid. The main connexions of the battery are made, and the principal current flows from 1 to 3, from 3 to 2, from 2 to 1. A diverted current may be led off from 1, and may pass in the direction *a b c d* through the galvanometer G, or sometimes in the opposite direction. At the same time an extra current may be established from C, the iron case of the cell 3, through the galvanometer G′, in the direction *e f g h i*.

The making or breaking of this extra current had no effect upon the diverted current *a b c d*, but the two were always in opposite directions. When the first moved the needle to the E. the second deflected its needle W. When the diverted current was W. the breaking the main current always turned it in the normal direction E, and at the same moment the needle of the extra current changed to the W. Upon restoring the main current both needles returned to their former position.

When the battery current, instead of being allowed to flow through short connexions, was led through a separate galvanometer, each of the other currents passing also to separate galvanometers, the diverted current varied in the different cells from 45° to 20°, but was always in the normal direction,

and the extra current was opposite. When the short connexion was added to the battery, as well as the long one through the galvanometer, the latter fell from 90° E. to about 20° E., and the diverted current oscillated rapidly E. and W., and the needle of the extra current changed with it in the opposite directions W. and E.

I could now have no doubt that the explanation of the sudden stoppage and reversal of the battery current, of which I was in search, was to be found either in this diversion which I have described, or from the opposition of extra currents exalted in their power by heat, or possibly from some combination of the two. I therefore returned to the original combination of the battery in the steamer.

I soon ascertained that the extra current could be produced by a connexion from any part of the tin case to any of the cells of the battery standing upon wooden blocks, and that its energy was increased both by acidulating the condensed water and by heat. I found also that by leading this current through the same galvanometer and voltameter as the battery current, that it interfered with it in different degrees, even to its stoppage and reversal. I must not attempt to give you the details of the numerous series of observations which I have made upon the subject, but will content myself with stating as concisely as possible the results of the last combination, which have proved always constant.

At figs. 66, and 67, I have represented, as before, a section of the arrangement: the course of the main current is marked by the arrow heads, and is conducted by the electrodes through the voltameter V and the galvanometer G. The battery was first charged in the usual way, and the cells were placed in the steamer upon blocks of damp wood standing in a little acidulated water. The observation commenced at the temperature of 52°, and the following are the tabulated results.

| Time. | Interval. | Voltameter. | 5 min. rate. | Galvanometer. |
|---|---|---|---|---|
| h m | ′ | Cubic inches. | Cubic inches. | ° |
| 10 58 | | | | |
| 11 3 | 5 | 6 | 6 | 60 E. |
| 11 5 | | | | |
| 11 10 | 5 | 12 | 6 | 60 E. |

A connexion was now made between the tin and the zinc cup of the galvanometer, as in fig. 66, by *a b c*, and the action was decreased.

| Time. | Interval. | Voltameter. | 5 min. rate. | Galvanometer. |
|---|---|---|---|---|
| h m | ′ | Cubic inches. | Cubic inches. | ° |
| 11 11 | | | | |
| 11 16 | 5 | 17 | 5 | 55 E. |
| 11 21 | 5 | 21¼ | 4¼ | |

The cover of the steamer was next put on, and the connexions made as before by the electrodes passing through the cocks.

| Time. | Interval. | Voltameter. | 5 min. rate. | Galvanometer. |
|---|---|---|---|---|
| h m | ′ | Cubic inches. | Cubic inches. | ° |
| 11 40 | | | | |
| 11 45 | 5 | 6·5 | 6·5 | 60 E. |

Connexion was again made from the tin (cover) to the zinc cup.

| Time. | Interval. | Voltameter. | 5 min. rate. | Galvanometer. |
|---|---|---|---|---|
| h m | ′ | Cubic inches. | Cubic inches. | ° |
| 11 50 | 5 | 11·5 | 5· | 55 E. |
| 11 52 | The steam was turned on. | | | |
| 11 57 | 5 | 19· | 7·5 | 65 E. |
| 12 2 | 5 | 27· | 8· | 67·5 E. |
| 12 7 | 5 | 36· | 9· | 70 E. |
| 12 9 | | | | |
| 12 14 | 5 | 11· | 11· | 77 E. |
| 12 19 | 5 | 21·5 | 10·5 | |

The tin was again connected as before.

| Time. | Interval. | Voltameter. | 5 min. rate. | Galvanometer. |
|---|---|---|---|---|
| h m | ′ | Cubic inches. | Cubic inches. | ° |
| 12 24 | 5 | 25· | 3·5 | 40 E· |
| 12 29 | 5 | stopped. | | 0 |

Now we may observe that in this arrangement the extra current, which we have already found completing its circuit, according to circumstances, either to the zinc or copper of the battery, has a path open to it by the wire F G to the zinc of No. 2, or through the galvanometer and voltameter, by the wire B A to the copper of No. 3, the latter being in opposition to the battery current. That in this series of experiments it tended to pass in the latter direction, is proved by the gradual retardation and ultimate neutralization of the latter. When the resistance to the main current was diminished by throwing the voltameter V out of the circuit by a wire passing from H to I, it again passed in the regular course, turning the needle of the galvanometer to the E.

When the secondary connexion was made with the tin by the copper cup C of the galvanometer, instead of the zinc cup, the battery current through the voltameter was again stopped, but the needle of the galvanometer turned 80° E., indicating a powerful current through it in the normal direction.

These experiments were frequently repeated with the same results.

In attempting to account for all the variable phenomena of these extra and diverted currents, it must not be overlooked

that it may be possible for the whole battery, or for a certain number of the series of the battery, to force itself a passage through the electrolyte on each side of the copper to the tin, and thus to discharge itself by what would appear to be a reversed current through the secondary communication. This would be determined by the amount of different resistances in the paths which might be open to it. In this way we can account for the different degrees of power in the extra current, and for its being able to pass through the voltameter at times and not at others.

The only difficulty in now accounting for the original stoppage of the battery current which I observed, consists in my not having been aware that there was any metallic communication between the tin and any of the battery connexions. I am now however convinced that, notwithstanding precautions were taken to avoid this, contact must have taken place. Indeed the distance between the connecting wires and the rim of the tin cover was but small, and the jarring occasioned by placing the cover in its place may easily have occasioned sufficient disturbance in the arrangement. This supposition also sufficiently explains why the stoppage could not invariably be produced when desired.

I think that I do not deceive myself in believing that the preceding observations may not be without interest and importance to those who are actively engaged in advancing by experiment our knowledge of one of the most wonderful and widely-diffused agencies with which matter has been endowed. If they should answer no higher purpose, they may very probably prevent the application of much labour and thought in the explanation of phenomena of a very striking but perplexing nature, which are very likely to be observed by those who are working in this field of inquiry, and of which in my own case the preceding pages are a very brief abstract. At the same time they afford an exemplification of the advantages of the *constant battery;* for both the diverted currents and the effects of temperature would have been masked and lost in the variable results of the common voltaic combinations.

The effects of heat upon single voltaic circuits have been ably investigated both by M. Marianini* and by Mr. Rogers;† but although both these gentlemen purposed to extend their observations to compound circuits, or the battery, they have probably been prevented by the cause which I have

* Annales de Chemie, Tom. XXXIII. p. 132.
† Silliman's Journal, January, 1835.

indicated. It is now, however, apparent that in the exact measures of different effects which an invariable current of electricity will enable experimentalists to undertake, the variations of atmospheric temperature even must not be neglected.

I remain, my dear Faraday,
Yours very faithfully,
J. F. DANIELL.

*King's College,*
*March* 1837.

POSTSCRIPT.

I have just completed a *constant battery* of large dimensions, the effects of which exceed my most sanguine expectations, and open new views of the possible application of the extraordinary powers of the voltaic current to economical purposes. It consists of only ten copper cells 20 inches high, $3\frac{1}{2}$ inches diameter, as in the first battery. The interior partitions are formed by merely tying the open ends of the oxen's gullets to the rings of the colanders for supporting the sulphate of copper, and which are made deeper than before, and suspending them in the cells, to the bottoms of which they reach. These membraneous bags contain each rather more than a quart of the dilute acid. The zinc rods are of the diameter of $\frac{5}{8}$th of an inch, well amalgamated, and the connexions are made as before described. At the temperature of 67° this battery produces, in the voltameters which I have all along employed in these researches, 12 cubic inches of the mixed gases per minute, or 720 cubic inches per hour. Its powers of ignition are very great; and while it will maintain 6 inches of platinum wire $\frac{1}{100}$th of an inch diameter red hot, it will still decompose water at the rate of 14 cubic inches per five minutes. The permanence of this result is very striking.

When the battery is not in use the rods are taken out and wiped, and the membraneous bags carefully lifted out of the cells, emptied of their acid, filled with water, and suspended from a frame placed for their reception. By this treatment I do not find that they are liable to any change of texture or deterioration; and I have now membranes which have been in use for several months and are quite perfect. If the acid be perfectly washed out of them they may even be dried with impunity; but it is better to preserve them in a moist state, as when dry they are liable to crack. The acid solution of sulphate of copper remains in the cells without injury, and in ten minutes the battery, when required, may be brought into action. There is no reason to think that the limits of efficiency have yet been nearly attained, and the gullets could

easily be connected together so as to obtain bags of any required length. I scarcely, however, think that in simplicity and cheapness of construction the battery can be further improved.

J. F. D.

*King's College,*
*June the* 15*th,* 1837.

---

XXXIV. *Description and Use of a Dipping needle Deflector, invented by* Robert Were Fox, Esq. *By* Mr. T. B. Jordan, *Philosophical Instrument Maker, Falmouth.*

*To the Editor of the Annals of Electricity, &c. &c.*

Sir,

The data for the accompanying paper having been kindly furnished by Mr. Fox,* I now have the pleasure of forwarding it for insertion in your valuable Annals, in connexion with the description of the instrument which I have already sent you.

I am, Sir,
Yours very respectfully,
T. B. JORDAN.

Description of the Instrument.

Fig 47 and 48, plate V. are front and back elevations of the instrument, and fig. 49, a transverse section with the deflectors screwed in. The same letters of reference are used in each figure. AB is the azimuth plate on which the vernier plate turns either in the same plane or with the usual beveled edge, as in theodolites. In the best instruments this limb is divided on silver or platina, and is read off by two verniers. The vernier plate is furnished with two levels, and the leveling screws are fixed to the foundation-plate, as in the figures; or the instrument is made to screw on a stand similar to that used for field theodolites. *c d e* is a circular brass box containing the needle, N S, two graduated circles, *f g*, and a thermometer, *h i*. The axis of the needle is terminated by exceedingly fine and short cylindrical pivots which work in jewelled holes. It may readily be removed from its bearings by releasing the screw, B, at the back of the instrument, which admits of the front jewel coming forward sufficiently to remove the needle. The small grooved wheel C, on the axis, is intended to receive a

* We have long solicited Mr. Fox for a description of his Dipping Needle Deflector, being well aware that many of our readers would be very much interested in it. Edit.

thread of unspun silk, furnished with hooks for hanging the weights on in taking intensity observations, and the thermometer $h$ $i$, is intended to note the temperature at the same time.

The graduated circles $f$ and $g$ are adjusted to a perfect coincidence: the front one serves to direct the line of vision, and answers the purpose of a vernier.

At the back of the instrument (Fig. 48) there is another graduated circle adjusted to coincide with those in the box; but the zeros are generally placed on the vertical instead of the horizontal line, for the convenience of having the vernier arm independent of those carrying the deflectors.

The deflectors, $k$ $k'$, fig. 49, are two small cylindrical magnets, having their poles terminated by cones. After being magnetized and reduced to a standard intensity by heat, they are carefully packed in brass tubes; the outer tube of each is furnished with a screw by which it can be fixed to the arms, D or D$'$, at pleasure.

G H, fig. 48, is an acromatic telescope fitted with webs, similar to a transit. Its use is to determine the true meridian for the variation of the needle; but as it has a vertical movement about the centre of the needle box, and a horizontal one on the vertical axis of the instrument, of course it may be used for measuring angles in either direction. It may be removed from its Ys when not in use.

The whole instrument is packed in a neat mahogany case, with magnetic armature, arranged so as to preserve the power of the different magnets. In addition to the parts described, the box contains a set of decimal weights for intensities, a pair of plyers, a rubber for producing friction on the back screw or pin, a screw driver, &c. &c.

The dotted lines in fig. 49 show the place of the telescope when in use.

## Use of the Instrument.

### *Reading off, by means of two parallel graduated circles.*

The graduations on the parallel circles being coincident, they serve to direct the line of sight, and to prevent parallax in determining the position of the needle.

This arrangement also answers the purpose of a *vernier:* thus suppose the outer graduated circle to be *fifteen times* further from the points of the needle than the inner circle, the lines of sight, of which the points of the needle form the respective pivots, must pass over *fifteen* divisions on the former, to cause the needle to appear to pass over *one* equal division on the latter. If the relative distances of the two graduated surfaces from the points of the needle are unknown, it is

evident that they may be readily ascertained by the same method.

To make this more clear, let the lines A B, fig. 48′, Plate V, represent portions of parallel circles divided to half degrees; $n$ the point of the needle, and $c\,d$ and $e\,f$ visual rays; then by inspection of the figure, it becomes evident that the eye must, in passing from $c$ to $e$, run over fifteen divisions on the outer circle before it can make the point $n$ appear to pass over one on the inner circle, so that the value of each division on the outer circle is two minutes, or of each degree four minutes. In reading off an instrument with this adjustment, we first observe the division which the needle has passed (in the figure this is 21°), and then carry the eye on until we find some division which will fall in the same line with it and the needle point; in the figure this line is $c\,d$, and the number of divisions passed over from 21° is 7, or 3° 30′, which makes the reading 21° 14′.

*To find the magnetic declination.*

Level the instrument by means of its screws and ascertain the true meridian by the telescope or sights at the back, as the case may be, these being parallel to the plane of the needle, and note the angle indicated by the vernier on the horizontal limb. Turn the box round till the needle stands perfectly vertical, gentle friction* having been several times employed to cause the needle to take its true position.

The friction is produced by an ivory or brass surface being rubbed against the extremity of a pin which projects from the back of the extremity of the jewel plate, and again note the angle on the horizontal limb. The face of the instrument should then be turned round to the opposite quarter till the needle again becomes vertical, and if it required more or less than 180° to effect this, half the difference will indicate the true magnetic meridian, the face of the instrument being at right angles to it.

*To ascertain the dip.*

The face of the instrument having been made to coincide with the plane of the magnetic meridian, by turning it 90° from its last position, and vibration produced as before, the mean indications of both poles of the needle should be carefully observed and noted. The face should then be turned round 180°, so as to be again in the plane of the magnetic meridian, and the observations repeated and noted, the mean of the whole will indicate the dip.

* This must in no case be omitted previous to reading off the place of the needle.

### *To correct the observed dip.*

The instrument being still in the plane of the magnetic meridian, screw on the deflectors *k k*, at right angles to the back, as shown in figure 49, so as to *repel* the ends of the needle which are nearest to them: adjust the deflectors at a given angle from the supposed or observed dip, say at 45° or 50° from it, as shown by the vernier or verniers at the back.

Suppose after vibration, that the lower end of the needle settles at 115° *from the o* on the north side of the instrument. Then turn the deflectors back to an equal angle from the observed dip, say 45° or 50° as the case may be, on the opposite side of it, which suppose to be 69° 20′, and that the poles of the needle stand at 23° 30′. Then 23° 30′+115°=138° 30′; which divided by 2 gives 69°15′. Turn the instrument round 180° in azimuth and repeat the process, and if the result should be 69° 13′ the mean or corrected dip will be 69° 14′. Similar observations may be multiplied at pleasure by varying the angles of the deflectors from the observed dip, but generally three sets of observations will be perfectly satisfactory.

Mr. Fox generally employs one deflector to ascertain the true dip, as small angles seem often to give more uniform results than large ones. The following may be considered as fair specimens of the results obtained by this method, even with a four-inch needle.* They were recently observed at Westbourne Green, Paddington, near London, by Mr. Fox, in company with Captain Ross, R. N.

The deflector which repelled the north end of the needle having been screwed into the lower arm at the back, and adjusted at 40° from the apparent dip of 69° 20′ first on one side and then on the other.

### *First Observation.*

| | | |
|---|---|---|
| Instrument facing East needle repelled to . . . | 87° 10′ | |
| On altering the deflector to 40° on the other side needle stood at | 51 20 | |
| Sum | 138 30; which ÷ 2 = 69°15′ | Mean 69° 17′·5 |
| Facing West needle repelled to. . | 86° 58′ | |
| And afterwards to . | 51 42 | |
| Sum | 138 40; which ÷ 2 = 69°20′ | |

* Satisfactory as these observations are, a longer and heavier needle is found to give still more uniform and consistent results.

*Second Observation.*

| | | | |
|---|---|---|---|
| Deflector adjusted at 50° from apparent dip of 69° 20′ facing East . . . | 85° 20′ | | |
| And afterwards . . | 53 8 | | |
| Sum | 138 28; | which ÷ 2 = 69° 14′ | Mean 69° 16′·5 |
| Facing West. . . . | 85° 2′ | | |
| And afterwards . . | 53 36 | | |
| Sum | 138 38; | which ÷ 2 = 69° 19′ | |
| General Mean . . . . . . . . . . . | | | 69° 17′ |

If the apparent dip had been taken at 69° 14′ or 69° 15′ the corrected dip would probably have been reduced to 69° 16′ or 1′ less, an original error of 5′ or 6′ in the apparent dip, being reduced to 1′ by this method of correction. It may here be observed that in this way the bearings of the axle of the needle are changed, and by turning the jewel plate the resting places in the jewels are also changed, and under all these circumstances the results have been found most satisfactory and remarkably uniform.

*To ascertain the terrestrial magnetic intensity by means of weights.*

Take off the deflector or deflectors and place the fine silk, with the hooks attached, on the grooved wheel, and suspend weights to one of the hooks so as to coerce the needle to a given distance, say 50°, from the actual dip at the station; and after applying friction as usual at the back, note the weights required; then change the weights to the other hook till the needle is coerced 50° on the opposite side. The weights required will indicate the magnetic intensity at the place of observation as compared with that ascertained in the same manner at any other place. Instead of having *given angles* to which the needle is to be deflected, it is more easy and practical to employ *given* weights, and the inverse ratio of the sines of the angles of deflection will give the intensity, corrections having been applied for differences of temperature at the different stations.

Mr. Fox has lately obtained many results of intensity, as well as of dip, at various places on the continent and in this country, with a small portable dipping needle deflector, having a 4-inch needle; and on this needle 1° of the centigrade scale produces an effect on the angles of deflection equal to 2′ to 2′·4. An example will show the method pursued.

Suppose two grains to be the given weight employed in one of his experiments, the instrument facing east, and that the needle counting from 0°, was stationary at 118° 8′ and afterwards on putting the 2 grs. on the other hook . . . . . . . . . at 20° 40′

| | |
|---|---|
| Included angle 97 28; which ÷ 2 = | 48°44′ |
| The same observations repeated facing W. | = 48 38 |
| Mean, instrument facing E. & W. | 48 41 |
| Temperature 17° centigrade, or taking 14° as the standard, there would be 3° in excess, for which deduct . . . . . . . . . . . . . . | 0 7 |
| Corrected mean angle | 48 34 |

the inverse ratio of the sine of which will show the intensity in relation to other results similarly made, and with the same weights.

Similar experiments may be made with other given weights, 2·1 grs., 2·2 grs., 2·3 grs., &c. for instance, and the number of observations multiplied at pleasure, and the mean of the whole taken.

The following are a few of the results which Mr. Fox has obtained, which may serve to show the working of the method in question. The needle used was only 4 inches long, and, consequently, did not give such uniform results as a larger needle would have done.

*Mean results obtained at different stations near London.*

Corrected angle, the instrument facing East and West.

| Grs. | | | Intensity. |
|---|---|---|---|
| 2·0 = | 48° | 36′·7 = | 1·0000 |
| 2·1 = | 51 | 55 ·3 = | 1·0000 |
| 2·2 = | 55 | 33 ·0 = | 1·0000 |

Eastbourne, in Sussex, Grounds of Davies Gilbert Esq. (Chalk.)

| Grs. | | | Intensity. |
|---|---|---|---|
| 2·0 = | 48° | 57′ = | 0·9938 |
| 2·1 = | 52 | 19 = | 0·9921 |
| 2·2 = | 55 | 57 = | 0·9952 |
| | | Mean | 0·9937 |

Eastwick Park, near Leatherhead, Grounds of David Barclay, Esq. (Chalk.)

| Grs. | | | | Intensity, |
|---|---|---|---|---|
| 2·0 | = | 48° 35′ | = | 0·9997 |
| 2·1 | = | 51 57 | = | 0.9996 |
| 2·2 | = | 55 40 | = | 0·9986 |
| | | Mean | | 0.9993 |

Grounds of Combe House, near Bristol, George Hilhouse, Esq. (Limestone.)

| Grs. | | | | Intensity. |
|---|---|---|---|---|
| 2·0 | = | 48° 25′ | = | 1·0023 |
| 2·1 | = | 51 45 | = | 1·0024 |
| 2·2 | = | 55 18 | = | 1·0031 |
| | | Mean | | 1·0026 |

Grounds of R. W. Fox, Esq., near Falmouth. (Clay Slate.)

| Grs. | | | | Intensity. |
|---|---|---|---|---|
| 2·0 | = | 48° 29′ | = | 1·0013 |
| 2·1 | = | 51 48 | = | 1·0017 |
| 2·2 | = | 55 20 | = | 1·0026 |
| | | Mean | | 1·0018 |

The intensity of the earth's magnetism may also be ascertained by means of the deflectors. For this purpose the latter should be screwed into the sockets in the arms at the back of the instrument and adjusted to the dip at the station, so as to repel the needle from it, first on one side and then on the other.* Half the sum of the included angles will represent the force of the earth's magnetism in relation to that of the deflectors on a needle thus circumstanced; that is, at the angles which the latter makes with them respectively. The value of the sines of angles thus taken at different stations and with the instrument facing east and west, may be ascertained by means of small weights. For this purpose the deflectors should be placed *at the angle* of which the value is to be ascertained, the same to be calculated from *the actual dip at the place of observation, where the weights are to be employed.* The weights required to coerce the needle back to

* The position of the needle may be readily changed from one side of the dip to the other by turning the jewel plate and its bracket by means of the knobs at the back.

the dip, against the force of the deflectors, will give that of the earth's magnetism on the needle, at the angle, the value of which is required, and in comparing the value of different angles the sines of which will be greater or less in proportion as the terrestrial magnetic intensity is less or more, corrections must be applied according to the sines of such angles in an inverse ratio. Such observations in order to be quite satisfactory, should be made with the deflectors adjusted on each side of the dip, and when convenient with the instrument facing the east and west, the mean of the whole being taken. Upon the whole, however, Mr. Fox prefers using the weights alone for ascertaining the intensity, if time should not admit of both methods being employed, which he finds they will do with a remarkable degree of uniformity and precision. It may here be observed that if the needle should have sustained any diminution of force it will settle at a *less* angle from the deflectors when in the dip at a given station, and at a *greater* angle when deflected by given weights only; whereas, if the force of the earth's magnetism should only be diminished by a change of station, the angles will be increased by both methods.

It is evident that if the deflectors are fixed at a constant angle from the dip, *at any given station*, and the needle is coerced, as before, into the line of dip, the weights required will be constant if no change has taken place in the magnetism of the deflectors or needle; and they will detect the amount of such change should it occur at any time or place.

In order to ascertain whether or not the needle itself has varied in intensity, remove the deflectors and screw the tube containing the second needle, which call number 2, into one of the arms at the back, so as for it to repel the suspended needle number 1. Adjust number 2 at any given angle, suppose 45° from the line of dip at the station, and coerce number 1 by weights into the dip. Repeat the operation with the opposite end of number 2, or rather screw the tube into the other socket. Half the sum of the weights required in both cases will indicate the repulsive forces of the needles with respect to each other under these circumstances at the angle of 45°. Remove number 2 from the back and deflect number 1 to any given angle, say 45° from the dip, by means of weights only, and note the weight required to effect this. Change the needles, placing number 1 in the tube, and suspending number 2, and go through precisely the same operations as before. If, under these circumstances, there should appear to be any difference from the previous results in the reciprocal action of the needles, take the mean of both.

It is highly desirable that the deflections should always be made on *both sides* of the line of dip to ensure accuracy, and the process will be still more complete if done with the instrument facing both east and west.

In this manner the reciprocal force of the needles on each other may be ascertained, and their respective forces in relation to that of the earth's magnetism under the circumstances described; and, therefore, approximately at least, the influence of any change in the former in relation to the latter, and vice versâ.

These relations may be ascertained by experiments and varying the force of the needles at a given station, where the terrestrial magnetic intensity is, for the time of their duration at least, presumed to be constant. And it can scarcely be doubted that this method furnishes the means of obtaining a true standard measure of the force of the earth's magnetism, at any time or place on given needles; or at least of approximating very closely to it.

In the course of such experiments it may, moreover, be found desirable to adjust the deflecting needle parallel to the arms at the back, (for which provision is made in the large instruments), and under such circumstances, to repeat the operations which have already been described, with the deflecting needle at right angles to the arms; and in both cases the given angles may be multiplied at pleasure; thus 40° and 50° may be taken as well as 45°, and the deflecting needle may be likewise fixed in the line of dip so as to repel the suspended needle from it instead of using the weights alone for this purpose, and in this way, the relation of the needles with respect to the earth's magnetism, may be ascertained by applying the weights as described in the case of deflectors.

It will not be requisite, at any time, to employ extremely minute weights; as the value of small differences in the angles may easily be estimated in weights; thus if $\frac{1}{20}$ of a grain should cause the needle, under given circumstances, to pass through 30′ $\frac{1}{200}$ of a grain, will be represented by 3′.

In all observations on the magnetic intensity the temperature should be noted, and the needful corrections applied, the amount of which may be readily ascertained by experiments, such as covering the instrument with a heated vessel inverted, or admitting heated air under it &c. In one instrument a needle when deflected by weights, 50° from the dip, had the angle increased more than one minute, by every degree (Fah.) of augmented temperature: in other needles of weaker intensity, the influence of temperature has been less considerable; the *ratio* appears to be nearly uniform within the ordinary range of changes in this climate.

If the needles are tempered very hard throughout, and, after having been magnetized, heated to 180° or 200°, Mr. Fox has found that they sometimes retain their force without any appreciable change for a long period of time, although he has continually observed that the magnetic axis* of a given needle, is liable to frequent variation even without its having been retouched.

This is shown by its having at one time uniformly an excess of dip, when facing east for instance, and at another, when facing west, and this without affecting the mean results on either occasion. He also finds that needles attain their maximum force after having been rubbed by a magnet or magnets two or three times only.

---

## XXXV. *Facts and Observations for the purpose of illustrating a Theory intended to connect the Operations of Nature, upon general principles. By* Paul Cooper, Esq.†

*(Continued from page 453, Vol. 2.)*

96. The principles of the theory which have been explained in the progress of this paper, appear to me to be clear and simple; but to those previously unacquainted with them, this may not be the case. To acquire a knowledge of any theory, which

* This fact suggests the expediency of having the wide part of a horizontal variation needle vertical, and not horizontal, to ensure the greatest degree of uniformity in its indications.

† Mr. Cooper has requested us to make ths following errata to his last paper, which appeared in No. 12, Vol. II. Edit.

"The figures in Plate XI. require the following corrections. In fig. 86 the particles of nitrate of lime ought to have the letter *l* over the lime, instead of the figure 1; and the particles of sulphate of potash ought to have the letter *p* over the potash, instead of the figure 1. The electro-motive force from the potash to the nitric acid, at the left side of the circle, is correctly marked 7; but the similar force between the two other particles of sulphate of potash and the adjoining particles of nitrate of lime, is marked 1 instead of 7. In the first case the figure 7 is erroneously introduced in six different places, and in the last case in two places.

"In fig. 80 the last atom of hydrogen ought not to be braced to the copper, this connexion being intended to distinguish atoms which are in a state of cohesion with each other.

"In fig. 77 the atom on the left ought to be marked oxygen, and that ou the right hydrogen; and the two atoms which are intended to exhibit a particle of water should be nearly in contact."

includes a large class of phenomena, it is necessary to become in some degree familiar with it, in order that its different bearings may be brought to the mind in a sufficiently concentrated point of view to remove the confusion which naturally arises upon the first presentation of a complicated subject. A watchmaker, or any other person well acquainted with the movements of a watch, sees at one view the connexion of its various machinery; but this machinery presents the greatest confusion to those who look upon its numerous motions for the first time. These considerations have induced me to illustrate the subject further, by showing the application of the same principles to some of the interesting experiments of Dr. Faraday; I shall introduce these experiments as concisely as possible, and give references to the sections in his Researches in Electricity, where they will be found at greater length.

97. Dr. Faraday prepared an apparatus, by plunging plates of platina and zinc into a vessel containing dilute sulphuric acid; these plates were not allowed to come in contact in the fluid, but were connected, by means of platina wires proceeding from them, through the medium of the fluid or other body, which they were intended to act upon. (Researches 899).

98. Water acidulated with sulphuric acid was not decomposed by the wires proceeding from this single pair of plates (Researches 903); but upon adding a few drops of nitric acid to the dilute sulphuric acid with which the plates were surrounded the decomposition of the water became evident. (Researches 973.) Iodine of potassium was decomposed by either of these preparations. The principles of these experiments, with the electro-motive forces which they bring into action, agreeably to the theory already set forth, (83) with the additional assumption that the electrical forces of oxide of azote and platina are equal, and consequently that their electro-motive forces are the same, are represented in figs. 68 and 69, Plate X.

99. Dr. Faraday says, "There is no point in electrical science which seems to me of more importance than the state of the metals and the electrolytic conductor in a simple voltaic circuit *before and at* the moment when metallic contact is first completed. If clearly understood, I feel no doubt it would supply us with a direct key to the laws under which the great variety of voltaic excitements, direct and incidental, occur, and open out various new fields of research for our investigation." (Researches 946.)

100. As I perfectly agree with Dr. Faraday upon the importance of this question, I shall endeavour to elucidate it in

the present enquiry. A knowledge of the state of the metals and the electrolytic conductor before and at the moment when the voltaic circuit is completed, will render the chemical action which follows perfectly simple and intelligible under every variety of circumstances.

101. The electrical state of the metals before contact is made is the same upon the whole of their surfaces; but when a plate of zinc is brought into contact with a plate of copper (or any other negative metal) there is a current from the copper to the zinc and the surfaces of the two plates assume the state represented in fig. 76, Plate XI. Vol. 2. When the circuit is completed by a conductor somewhat inferior to the actual contact of the plates, there is a current through this conductor from the zinc to the copper corresponding with that which passed directly from the copper to the zinc; but the plates, although each of them has now its natural proportion of light, still present positive and negative surfaces as they are described in the figure. (57.)*

102. Now, if we separate the plates, keeping them at the least possible distance from each other so that they be not in actual contact, and connect their external surfaces by the wire W, the state of the whole of the surfaces will be reversed and the current will flow from the copper to the zinc through the wire. If under these circumstances the circuit be completed by the interposition of water between the plates, as in fig. 77, the particles of water will be arranged as in figs. 77 and 78; the oxygen, the positive element of the water, being attracted by the *induced* negative surface of the zinc and the hydrogen, its negative element, by the *induced* positive surface of the copper (62. 144). The current upon thus completing the circuit, will be from the zinc to the copper through the interposed water; but as fluids under the polar arrangement here represented cease to be conductors except upon the principle of decomposition (67), the poles of the atoms become reversed by the current, the atoms of oxygen in contact with the zinc unite with it, and after the decomposition and recomposition of every intermediate particle of water in the lines of transmission, the last atoms of hydrogen are set at liberty. The quantity of light which is discharged at the cathode, and thus thrown into the form of a current, is precisely in proportion to the number of atoms of hydrogen which are separated at the surface of the copper; and exactly what is required to give positive surfaces to the oxygen at the anode to enable it to unite to the zinc.

* See page 364 of Vol. 2.

103. It may not appear obvious to those not well acquainted with the subject, why, in forming a particle, it is absolutely necessary, in order to give the union stability, that the positive atom should present a positive surface to the negative surface of the negative atom (63); but a little consideration will remove the difficulty. Let us suppose that the particle of water, represented in fig. 77, Plate XI. Vol. 2, instead of being formed of an atom of oxygen and an atom of hydrogen united by the positive surface of the former to the negative surface of the latter, should be formed by the union of the negative surface of the oxygen with the positive surface of the hydrogen; as it must be, supposing a cohesive union possible under these circumstances, upon the reversal of the poles of the electrolytic atoms in fig. 78. A particle thus formed is represented in fig. 70, Plate X.; and it will be evident from an inspection of it that there is the same quantity of light between the two atoms as in fig. 77, Plate XI. Vol. 2, but differently arranged. The instant, however, that such a particle is set at liberty from the coercive force by which it was formed, the light which forms the positive surface of the oxygen, the positive atom, finding a force of resistance in the atmosphere of the hydrogen, the negative atom, less than its own propelling force, will flow towards the hydrogen (without quitting its own atom) and at the same time repel the positive force of the hydrogen towards its negative surface (49); there will, of course, be a moment when the two atoms will be in a natural state, and at that instant they will assume their gaseous form. It is therefore evident that in a line of electrolytic atoms, the poles of which have been reversed, the first atom of the positive element and the last atom of the negative element must be set at liberty, unless they find at the electrodes elements with which they can enter into a new state of combination. The intermediate atoms will, of course, form particles in the natural order of their polar forces (69). These changes, let it be observed, are not made by a transfer of any cohesive force from the atoms separated to the atoms which form new combinations; but by a general transfer, in which the whole of the atoms alike participate, and which leaves every atom in possession of its natural forces differently arranged.

104. Our knowledge rested at the point we have just left, namely, that water interposed between plates of zinc and copper or zinc and platina was decomposed, provided a metallic communication was made between some other points of the plates, either by direct contact or through the medium of a metallic conductor (such as A B, fig. 78, Plate XI. Vol. 2), when Dr. Faraday made the discovery that metallic contact

might be altogether dispensed with;* that the wire A B might be broken and a fluid introduced, (as in figs. 68 69, and 71, Plate X.), when certain conditions with regard to the nature of the fluid are attended to, without preventing the production of the voltaic current. These conditions, as stated by Dr. Faraday, are, that the set of chemical affinities for the zinc in the fluid between the plates should have sufficient strength to overcome the resisting chemical affinities in the fluid introduced between the wires. (Researches 884. 891.)

105. I have already observed that the electrolyte in fig. 78, Plate XI. Vol. 2, takes the polar arrangement there described in consequence of the negative state of the surface of the zinc and the positive state of the copper *induced* by the communication ofthe opposite surfaces of the plates through the medium of the wire A B (144). Now, if this wire be broken at any point between the copper and the zinc, the inductive influence of the plates upon each other being preserved in the same order by the introduction of some other conducting body between the points of the wires thus divided, the point of the wire A proceeding from the zinc will present to the interposed body a positive surface, and the point B, proceeding from the copper will present to it a negative surface; the *induced* surfaces at the points of the wires will therefore be in opposite states to the *induced* surfaces of the plates which produced the arrangement in the electrolyte placed between them. If, then, the body interposed between the points of the wires be a fluid, its positive element will be turned towards the wire proceeding from the copper and its negative element will be directed towards the wire which proceeds from the zinc, as in figs. 68, 69, and 71, Plate X.; and this will be the case whether the wires be formed of the same materials with the plates from which they proceed, or of any other metals.

* We are much obliged to Mr. Cooper for reminding us of this discovery. We have long viewed it as an important fact in the theory of voltaic electricity. Dr. Faraday also saw the importance of this fact at least four years before he *ventured* to claim it as his own discovery. He has made it the theme of his Eighth Series, claiming all the credit of the discovery to himself. Dr. Faraday has now enjoyed the *unmolested* credit of this discovery more than four years; and as the fact itself has attained some degree of repute in the philosophical world, some further information respecting its history may possibly be interesting to many of our readers. Perhaps they will hardly suspect that Dr. Faraday made the discovery in a pamphlet which was published in 1830: and that the said pamphlet was consulted at the time he was writing his Eighth Series.—We hope Dr. Faraday will be as prompt on this, as on a former occasion, in taking advantage of his favorite motto, "Delays are dangerous." Edit.

106. The manner in which the polar forces are propagated in the wires is shown in figs. 71 and 72. The atoms of the wire are all arranged so as to prevent alternately positive and negative surfaces in the direction in which the current is to be transmitted; the wire, therefore, in this state is as much a non-conductor, as the fluid under the influence of polar surfaces represented in fig. 79, Plate XI. Vol. 2. But in the present case, there being no natural difference of capacity in the atoms of the wire, the whole of the positive surfaces and the whole of the negative surfaces are of uniform intensities, and if they could be turned at right angles to their present position would both form channels of light in right lines for the transmission of the current; this, however, is opposed by the inductive influence and polar attraction of the plates, and the polar surfaces of the atoms, taking a middle course, unite so as to form spiral lines of uniform intensities in the direction in which the current of light is to be transmitted. These spirals are formed at different angles under different circumstances; and when the galvanometer is introduced into the course of the current it indicates the inclination of the polar surfaces.

Hence it is that bodies which are conductors of electricity upon the principle of decomposition when fluid, generally become non-conductors when solid; the different capacities of their atoms, which is essential to their electrolytic arrangement, being opposed to their forming spirals of uniform intensities. Dr. Faraday found that in almost every instance bodies which conduct a voltaic current when fluid, and refuse to conduct it when solid, are decomposable by electricity. (Fourth Series of Researches.)

107. But there is one condition which is essential to the arrangement, as they are described in the figures; the fluid introduced between the points of the wires A and B must be a *better* conductor of the inductive forces of the plates than the fluid which is interposed between the plates themselves; for if the latter be a better conductor of these forces than the former, the whole of the arrangements will be made in the reverse order, and the current will also be reversed. If, for instance, a wire be introduced between the zinc and platina plates, fig. 71, as represented in fig. 72, the polar arrangements will be reversed; the wire A will present a negative surface and the wire B a positive surface to the iodine of potassium; the iodine will be directed towards the wire A and the potassium towards the wire B; and the current will be from A to B, with decomposition of the iodine as before (115). (Researches 882.)

108. It appears, then, that the direction of the current depends in a great measure, if not wholly, upon the conducting

qualities of the bodies by which the opposite surfaces of the plates are connected; that when both these bodies are electrolytes, that which presents the weakest set of chemical affinities is the best conductor and determines the course of the current which passes through it to be from the platina to the zinc, being the same direction as when the communication is made by metallic contact. It will be seen that the latter part of this statement, as it relates to the chemical affinities of the two electrolytes, is made in accordance with the opinion of Dr. Faraday. (Researches 891.)

109. I think I perceive in this, a clue to the solution of the difficulty I have already stated, with regard to the origin of the electro-motive force in the voltaic current (92). Two things appear to me to be very evident; the first is, that, in accordance with our general law, the current is always from the negative surface of one metal to the positive surface of the other: the second, that the opposite surfaces of the plates, in order to produce a polar arrangement, must be connected by conductors of different conducting powers. The best conductor of the inductive forces establishes a positive polarity on the surface of the positive metal and a negative polarity on the surface of the negative metal, when they are connected by it; the opposite surfaces of the two metals being, of course, in opposite states. The current, then, with whatever remains of its original force, constantly proceeds from the negative metal through the *best conductor* to the positive metal, and from the positive metal through the *inferior conductor* to the negative metal, as in fig.71 and 72, Plate X. Now, if we refer to figs. 82 and 83 Plate XI. Vol. 2 (or to any others which are explained upon the same principle) and reduce the electro-motive force of copper to zinc from two to one, being the difference of the forces of hydrogen and zinc for oxygen, it will bring the electro-motive forces equal in both directions; but the metallic communication between the copper and the zinc forms a better conductor of the inductive forces of the metals than the electrolyte by which their opposite surfaces are connected, and, conformably to the preceding observations, a positive polarity is induced upon that surface of the positive metal which is in metallic communication with the negative metal, and a negative polarity upon the surface which communicates with it through the medium of the electrolyte. In accordance, then, with the law before referred to (54), the current is from this negative surface through the fluid, with decomposition, to the positive surface of the negative metal, as indicated by the arrows.

110. That the current is always from the negative surface of one metal to the positive surface of the other, is a necessary

result from theoretical considerations (52); and it has been confirmed by every experiment referred to in the course of this investigation which has been calculated to display it. That the opposite surfaces of the plates, in order to produce a polar arrangement, must be connected by conductors of different conducting powers will be apparent from a little consideration. When, in the experiments of Dr. Faraday, plates of platina and zinc were plunged into a vessel containing dilute sulphuric acid, without being in contact, the forces on the surfaces of the plates were so exactly balanced that no arrangement of this kind could take place. When these plates were connected by platina wires proceeding from them, through the medium of a fluid of the same kind as that contained in the vessel (Researches 903.) the circumstances were not at all changed, the surfaces were still in communication through the medium of conductors of equal powers, and the electrolytic arrangement in fig. 98, representing the position of the particles of water and the state of the different surfaces of the metals which would have been required to produce the decomposition of the former, did not, in fact, take place. Hence the fluid in such cases continues to be a conductor of electricity, as under ordinary circumstances, when it is not exposed to the influence of polar forces.

Our voltaic batteries are all formed upon the principle of connecting the opposite surfaces of the plates by conductors of different powers; the connexion being generally metallic on one side, and through the medium of the electrolyte on the other. In the dry pile, this principle is preserved by bringing the metals into contact on one side and interposing paper between them on the other. The current in all these cases, in accordance with the general law, is from the negative to the positive surface.

111. Dr. Faraday varied the experiments we have referred to, in the following manner. "Let two plates, one of amalgamated zinc and the other of platina, be placed parallel to each other (fig. 73), and introduce a drop of dilute sulphuric acid, *y*, between them at one end: there will be no sensible chemical action at that spot unless the two plates are connected somewhere else, as at P Z, by a body capable of conducting electricity. If that body be a metal or certain forms of carbon, then the current passes, and as it circulates through the fluid at *y*, decomposition ensues." (Researches 889.)

112. Then remove the acid from *y*, and introduce a drop of iodide of potassium at *x*, fig. 74. Exactly the same set of effects occur, except that when the metallic communication is made at P Z, the electric current is in the opposite direction.

to what it was before, as indicated by the arrows, which show the courses of the currents." (Researches 890.)

113. "We find that when the solutions are used at opposite ends of the plates, as in the two last experiments, metallic contact being allowed at the other extremities, the currents are in opposite directions. We have evidently, therefore, the power of opposing the actions of the two fluids simultaneously to each other at the opposite ends of the plates, using each one as a conductor for the discharge of the current of electricity, which the other tends to generate; in fact, substituting them for metallic contact, and combining both experiments into one (fig. 75)." (Researches 891.)

114. The figures are drawn upon a much larger scale and in different proportions to the corresponding figures of Dr. Faraday, in order to show the manner in which the polar surfaces of the atoms of the different elements are connected with each other. In fig. 73, the metals being connected by a wire at P Z, a positive surface is induced upon the zinc, and a negative surface upon the platina, at the points at which the wire is in contact (106). The polarity thus induced is propagated through the two metals, from atom to atom, until it reaches the acidulated water at $y$, where the zinc, of course, presents a negative surface, and the platina a positive surface; the former, therefore, attracts the oxygen, the positive element; and the latter the hydrogen, the negative element of the water, producing the arrangement described in the figure. The course of the current is from the negative surface of the platina through the wire, to the positive surface of the zinc, and through the zinc to the point at which it presents a negative surface to the fluid at $y$; passing through which, with decomposition in the manner already described (69), it again enters the platina to complete the circuit.

115. In fig. 74, every thing is precisely the same with the exception of the substitution of the iodine of potassium for the acidulated water; the iodine, the positive element, occupying the place of the oxygen, and the potassium, the negative element, taking the place of the hydrogen. The current is in the *opposite direction* merely because the metallic communication is made at the opposite ends of the plates, as may be seen in fig. 74; and the decomposition of the iodine of potassium is effected, in the usual manner, by reversing the poles of the atoms which form the lines of particles between the zinc and the platina; the terminating atoms of the iodine being united to the zinc and the terminating atoms of the potassium separated at the surface of the platina. (See Researches 882).

116. In fig. 74, the iodine of potassium occupies the place of the wire in the two former experiments; but it is here presented to a positive surface of the zinc and a negative surface of the platina, and the potassium is in consequence directed to the former, and the iodine to the latter. (See Researches 881). The current being now from the platina to the zinc, the iodine is separated at the surface of the platina, and the potassium at the surface of the zinc, the whole of the arrangements with regard to its decomposition being reversed (104). If the wires A and B in figs. 71 and 72 are formed of the same metals as the plates from which they proceed, the experiments to which they refer will be exactly similar in principle to those we have here explained. I need not say that although only single particles of the fluids are represented between the plates at $x$ and $y$, the spaces contain many thousands, all of which have the same directions as those described.*

117. If we adopt the explanation of the origin of the electro-motive force as it is stated in a former part of this paper (83), we must conclude that the current is put in motion in fig. 73 by a balance of force arising from the decomposition of the water and the union of its oxygen with zinc. In fig. 74 the force must arise from the decomposition of the iodine of potassium and the union of its iodine with the zinc. In fig. 75 the force must arise from the same cause as in fig. 73; but as the iodine of potassium is in an opposite direction to what it was in fig. 74, the balance of its electro-motive forces will be reversed, and a part of the force arising from the decomposition of the water at $y$, will be expended in overcoming the resistance of the iodine of potassium to decomposition at $x$ (142). If, on the other hand, we suppose the surplus electro-motive force to be wholly derived from the plates (108), this force must be greatest when the current is conveyed through an unresisting medium, as in fig. 76, Plate XI. Vol. 2; for if there be no balance of force arising from the decomposition of the electrolyte, the indisposition which all bodies have to undergo a change of state must produce resistance, and the current will pass through the wire A B, fig. 78, Plate XI. Vol. 2 with less force than through the wire in fig. 76. If, again, the wire A B be broken, as in fig. 71, Plate X., and another electrolyte introduced, its decomposition, even supposing no balance of

* The elliptic form given to the circuit in these figures is merely intended to show the manner in which the different elements are connected; and not with any view to the actual form of the curve on these occasions, which I am of opinion always approaches, as nearly as circumstances will allow, to the magnetic curve.

electro-motive force to be opposed to it, will produce a further loss of force in the current.

It appears to me, however, that the loss of force produced by decomposition, arises from the reverse order in which the electrolyte is *always* presented. By referring to fig. 73 we may observe that the oxygen presents a negative surface to a negative surface of the zinc, and the hydrogen a positive surface to a positive surface of the platina, whereas the wire presents a positive surface to a negative surface of the platina, and a negative surface to the positive surface of the zinc; the latter, therefore, offers no resistance to the propagation of the inductive forces of the plates from one to the other, while the former, being in the reverse order, is presented under similar circumstances to a reversed pair of plates in a voltaic battery, which requires to be brought into the proper order before they can transmit the inductive forces of the plates with which they are associated. (Researches 1046.) Thus, the loss of electro-motive force in the current increases with the increased affinities of the elements of the electrolyte which is decomposed, as the loss of the same force increases in the battery with the increased difference of electrical force in the plates which are presented in the reverse order. It appears probable, therefore, that decomposition is, under all circumstances, effected at the expense of a part of the electro-motive forces of the two metals which the electrolyte is the means of bringing into communication with each other to form the circuit (141, 142).

118. Whichever of these explanations be adopted, the continuous character of the current must be attributed to the breaking of the circuit when the decomposition of a particle of the electrolyte is completed, in order to form a new arrangement that the process may be repeated (69). Upon the circuit being thus interrupted, the plates return to the intermediate state which would arise from the removal of the wire in fig. 76, Plate XI. Vol. 2 (58); and upon its being again completed, by the replacing of the particles of the electrolyte in their former order, the current will be renewed, as it would be by replacing the wire in the figure referred to. The general current is not affected by these interruptions, because it is supplied by the numerous atoms upon the surfaces of the plates, each of which upon one plate is connected with an atom upon the surface of the opposite plate, by a line of particles in the electrolyte, so as to form a circuit independent of every other circuit; one part, therefore, supplies the current while the circuits of the other part are broken and in a state of preparation. Chemical action, then, appears to produce a constant current, simply, by alternately breaking and completing these circuits with great

rapidity; the acid, alkali, or salt, usually added to water, being of no other use than to facilitate the necessary arrangements (70, 130).

119. The following interesting experiment appears to me calculated to throw considerable light upon the part of this subject which relates to the conduction of the inductive or deranging forces. "Dr. Faraday took a voltaic apparatus consisting of a single pair of large plates, namely, a cylinder of amalgamated zinc, and a double cylinder of copper. These were put into a jar containing dilute sulphuric acid, and could at pleasure be placed in metallic communication by a copper wire, adjusted so as to dip at the extremities, into two cups of mercury, connected with the two plates. Being thus arranged, there was no chemical action whilst the plates were not connected. On making the connexion, a spark was obtained, and the solution was immediately decomposed. On breaking it, the usual spark was obtained and decomposition ceased." (Researches 956, 957.)

120. In this experiment it is evident that the circuit which conveyed the inductive forces, was completed, through the medium of the air between the points of the wires, before the electric current was produced; and that the metals and the electrolyte acquired from it what Dr. Faraday has very happily called a state of tension. (Researches 956.) Every part of the circuit was in possession of a force exerting itself to produce the changes which could only be completed upon the passing of the current; and it proves very clearly, what I have elsewhere stated* (20), that the inductive force is conveyed perfectly independent of the current, the latter being a consequence and not the cause of the former.

121. The force of the electric current in every case, both in electrics and conductors, is produced by the state of tension arising from a previous transmission of the inductive or deranging forces. There is, consequently, no proof in the present experiment that the air is a better conductor of induction than of the light which is required to complete the derangement, because the former necessarily precedes the latter, even supposing the conducting power of the air to be equal for both; but we have this proof in a variety of other experiments. When, for instance, we bring a body charged with electricity within a short distance of an uninsulated conductor, the deranging influence of the electric is conveyed through the air; but the current of light required to complete the inductive process takes place between the conductor and the ground

* Abstract 16.

with which it is in communication. When, again, the conductor is insulated and placed at the same distance from the changed body as in the last experiment, the state of tension which preceded the current must be produced as before; but the insulated position of the conductor, prevents its communication with the ground, and as there is no current under these circumstances between the charged body and the conductor, we may conclude that the air is a better conductor of the deranging force than of the light which is necessary to complete the derangement; the conduction of the former being proved by the first experiment, and the want of conduction for the latter by the last. Hence the cause of the spark in Dr. Faraday's experiment becomes evident; the inductive force being conveyed a greater distance through the air than that through which the current would pass, produced a state of tension in the circuit; and to satisfy this, the current passed through the air upon the points of the wire coming within the distance at which the light could be transmitted by it. On breaking the connexion the circuit returned to its previous state (58), and the current necessary to complete this state passed through the air as before, but in a contrary direction. When the inductive force is conveyed through glass or any other electric which altogether prevents the passing of the current, the independent character of the deranging force becomes still more evident.

122. While I am writing this, an abstract of Dr. Faraday's eleventh series of "Experimental Researches in Electricity" has been put into my hands.* From this abstract I am glad to learn that Dr. Faraday has turned his attention to the interesting subject in which we are at present engaged; but I am still more gratified to find that he has been led to the conclusion, "that common induction is in all cases an *action of contiguous particles*, and that electrical action at a distance, which is what is meant by the term induction, never occurs except through the intermediate agency of intervening matter." And, also, that he is convinced "that induction is the result of actions among the individual and contiguous particles of matter, having both forces developed to an extent exactly equal in each particle.'

123. That induction is in all cases the result of a succession of actions between contiguous atoms of matter; that electrical induction, in common with every other kind of derangement, is extended to bodies indiscriminately, from atom

* Dr. Faraday's Eleventh and Twelfth Series of Experimental Researches will soon appear in these Annals. Edit.

to atom, until the force is exhausted, consequently, that it is conveyed to a distance through the intermediate agency of intervening matter; and that the positive and negative forces are equally developed in each atom, except in cases where this equal development is prevented by the resistance of other forces, are some of the leading principles of the theory I am now endeavouring to illustrate.

124. The enquiring and penetrating mind of Dr. Faraday will not long rest here. From a knowledge of the equal development of the two forces in each particle, he will be led to observe that the particles become neutralized by the transfer of something connected with these forces from one side of the particle to the other, and he will naturally conclude that this is the electric fluid. He will then perceive that this fluid has at least some of the properties of common light; that if the air be made part of the medium through which it is transmitted, the dissipation of light on its passing through this medium, will as usual take place; and if the experiment be made in a room otherwise dark, objects will be seen by it as with light from the sun or from a candle transmitted by the same medium. He will then, perhaps, recollect that the fluid transferred from one side of the particle (atom) to the other still adheres to it; and as it is a matter of indifference which side of the particle (atom) is presented to the inductive force, he will naturally conclude that it forms a kind of atmosphere which surrounds it. The recollection that this atmosphere is formed of a fluid, the atoms of which are known to repel each other, will probably suggest the idea that it may be the cause of the elasticity of bodies, by keeping their atoms and particles at certain distances from each other. These distances, then, being increased by the addition of caloric, it would be unphilosophical to introduce two agents to perform the same operation; and the inference will be, that the electric fluid and caloric are the same. But if the caloric be added beyond a certain quantity, the repulsive force of its atoms will put in motion the similar atmospheres of surrounding atoms and give it the form of a current, either as radiant heat, or, when of greater intensity and projected with greater force, as light. It will thus be readily discovered that light and caloric are the same,* the one in motion the other at rest; and that the

* Sir H. Davy ought to have drawn this conclusion when he discovered "that the heat of flames may be actually diminished by increasing their light, and vice versâ." Experiments and new views respecting Flame. Brande's Journal, Vol. II. p. 124.

The experiments on which this discovery was founded are now very generally applied in the combustion of coal gas for the pro-

phenomena of electricity are produced by the unequal distribution of this fluid upon the surfaces of the atoms to which it is attached.

125. If these observations should meet the eye of Dr. Faraday I hope he will excuse the liberty I have taken in making use of his name so freely. I can assure him that, notwithstanding he has passed over my humble labours unnoticed, it has given me unmixed satisfaction to find that after travelling the same road many years in opposite directions, he having proceeded from experiment to theory, and I, by a process, I trust, equally inductive, from theory to experiment, we have at length crossed each other's path; and I take this opportunity of acknowledging the great and essential advantages I have derived from his beautiful and masterly experiments, by appealing to them as tests of my own theoretical views.

126. The following experiments, though they do not introduce any new principle, will place those we have already investigated in a somewhat different point of view.

Dr. Faraday divided a cylindrical glass cup down the middle into two halves, and after introducing bibulous paper between them pressed the two halves together by means of a brass ring so as to render the cup water tight. The two spaces thus produced were called the cells A and B. Dilute sulphuric acid was put into the cell A, and a strong solution of caustic potassa into the cell B; a plate of clean platina was then put into each cell and connected with a delicate galvanometer; but no electric current could be observed. (Researches 937, 938.)

127. "When one of the platina plates was removed and a zinc plate substituted, either amalgamated or not, a strong electric current was produced. But whether the zinc were in the acid whilst the platina was in the alkali, or whether the reverse order were chosen, the electric current was always from the zinc through the electrolyte to the platina, and back through the galvanometer to the zinc, the current seeming to be the strongest when the zinc was in the alkali and the platina in the acid." (Researches 939.)

128. The acid being positive to the alkali, upon being placed in the two cells the particles of the solution would assume the form represented in figs. 76 and 77, the particles

duction of heat; it being found that as the light arising from the consumption of an equal quantity of gas is decreased, the heat is increased; that in order to retain it as heat we must prevent its radiation as light; in fact, that heat and light are mutually convertible into each other.

of the acid solution presenting positive surfaces to the alkali, and the alkaline solution negative surfaces to the acid. When plates of platina were put into both cells, as in the first experiment, the atoms of each plate would assume the same form as the particles of the solution in which it was placed; the plate in the acid would present a positive surface to that in the alkali and the plate in the alkali would present a negative surface to that in the acid. Under these circumstances there would probably be a feeble current from the negative surface of the plate in the alkali to the positive surface of the plate in the acid; but as there could be no chemical action to break the circuit, when the plates were brought into a state of equilibrium with each other the current would cease. When a zinc plate was substituted for the platina plate in the alkali, as in the second experiment, a current would take place from the negative surface of the zinc to the positive surface of the platina across the two solutions as before, but with greater force in consequence of the dissimilarity of the metals; and this current would be rendered continuous by the breaking and renewing of the circuits upon the decomposition of every particle of the water (117).

129. When this experiment was varied by substituting the zinc for the platina in the acid, instead of the alkali, the zinc plate presented a positive surface to the negative surface of the platina, and the current took place through circuits in the electrolyte, formed from the negative suaface of the zinc to the positive surface of the platina, and from the platina through the galvanometer to the positive surface of the zinc as before; but the circuits being circuitous, as represented in fig. 77, a greater resistance was opposed to the current and it was not so strong as in the preceding experiment.

130. A direct circuit across the two solutions, from the negative surface of the plate in the alkali to the positive surface of the plate in the zinc, gives such an advantage over the circuitous circuit we have just described, that when plates of zinc, tin, lead, or copper, were put into both solutions, the electric current was from the plate in the alkali across the cells to the plate in the acid, and back through the galvanometer to the plate in the alkali. The current in these cases is so powerful, that if amalgamated zinc, or tin, or lead, be used, the metal in the acid evolves hydrogen the moment it is placed in communication with that in the alkali. The advantage thus given is further proved by this, that if metals varying but little in their electrical forces be used, such as zinc and tin, or tin and lead, whichever metal is put into the alkali becomes oxidized, whilst that in the acid remains in a

metallic state, as far as the electric current is concerned. (See Researches 941, 942.)

131. These experiments are instructive, inasmuch as they show that a considerable electro-motive force may be generated without the aid of dissimilar metals, provided a polar arrangement be given to the plates by other means. The chief advantage derived from employing dissimilar metals, is, the production of this arrangement, without which there would be no motive for the particles of the electrolyte to take the direction required for their decomposition. In local circuits the polar arrangement is made by the inductive influence of the terminating atoms of oxygen and hydrogen communicating with each other through the same piece of metal, and when the circuits are thus formed they appear to be as effectual as those formed between dissimilar metals (73); but as the polar state of the surfaces is the result of an inductive influence, to obtain which the circuit must be completed, there can be no inducement for their formation in metals which are perfectly homogeneous. The superior chemical action of simple galvanic circuits, when compared with the action produced by local circuits, may in a great measure be attributed to the facilities presented by the former for making electrolytic arrangements (72); and it is probable that much of the advantage resulting from the addition of acids and salts to the electrolyte is derived from their forming circuits with powerful polar forces, which, though they are non-conductors of light, induce upon the surfaces of the metals with which they are connected corresponding polarities: and thus lead to the formation of similar circuits in the water, the polar forces of which, being less powerful, cannot form the necessary arrangement, particularly in local circuits, without this assistance.

132. The last experiment, in which zinc and tin, and tin and lead, were substituted for each other, proves, not only that an electro-motive force may be thus generated without the aid of dissimilar metals, but that it may be also produced in opposition to the natural relative forces of the two metals (139). The experiments by Sir H. Davy, which follow, give another instance of this. "If iron and copper be plunged into dilute acid, the current is from the iron through the liquid to the copper: in solution of potassa it is in the same direction, but in solution of sulphuret of potassa it is reversed. In the first two cases it is oxygen which combines with the iron, in the latter sulphur which combines with the copper, that produces the electric current." (Researches 943.)

(*To be continued.*)

## XXXVI. *Improvements in the voltaic battery.* By E. S. Clarke, Esq. *In a letter to the Editor of the Annals of Electricity.*

Palmerston, near Dublin,
August 13, 1838.

Dear Sir,

In the communication which I had last the pleasure of forwarding to you, I find that I merely enclosed the original paper, which I had the honour of reading a short time before to the Royal Irish Academy, on the double copper sustaining battery of my construction; and that I omitted to describe an alteration in the form of that instrument, which I had made subsequent to the reading of the paper referred to. And as this alteration renders the instrument rather more convenient, I take the liberty of sending you a very brief account of the same for insertion in your very valuable Annals.

It will be recollected that in the form described in my former paper, a cast zinc ring is secured by zinc rivets to the zinc cylinder of the battery, this ring being of use chiefly to enable a bladder to be secured upon the zinc cylinder, exclusively of that secured upon the copper cylinder, whereas the latter alone is used in the other forms of sustaining battery: but in the altered form which I now use, there is a loose ring of wood or copper of the shape shown at fig. 50, Plate V. To this a bladder is secured by drawing it inside of the wooden ring, and then after turning its upper edge over the top of the ring, tying a string round it in a groove formed in the exterior of the ring, when the latter, thus furnished with its bladder, assumes the form shown in perspective at fig. 51, and in section at fig. 53. This ring and its bladder is then to be placed within the external copper case, fig. 52, where it is supported by the shoulder, at *a*, formed therein about one inch and a half below its top. And after having been charged as before, with muriate of ammonia, the zinc cylinder is placed within it without having any zinc ring attached to it.

This separation of the bladder from the zinc cylinder, by means of a loose wooden ring, is convenient for different reasons: first, as each battery may have two such rings, on which bladders may always be kept tied ready for action; and again it prevents the possibility of an occurrence which may take place with the attached zinc ring, except the projecting part, B, of fig. 52, be made pretty large, I mean contact between the zinc and copper cylinders.

The second difference in form between the battery which I now use and that described in my former paper, consists in the addition of a mouth, M, to the exterior copper vessel as shown at fig. 52. This mouth has at its bottom a tube of $\frac{5}{8}$ of an inch diameter, communicating with the interior of the vessel; and it also has a perforated false bottom or shelf, on which crystals of sulphate of copper are placed, in order to ensure a state of saturation in the cupreous solution; besides which purpose it shows when a sufficient charge has been put into the exterior copper vessel, and affords a facility in charging or removing a charge from the latter.

The false bottom of the mouth, M, in fig. 52, is level with the shoulder, *a*, which itself is about 1 inch below the top of the zinc cylinder, as will be seen in the sectional sketch of the entire, shown at fig. 54.

Having now noticed the alteration made since my last communication, and thus shown the form of battery which I now generally employ, I may add that I sometimes have the interior copper not closed at top as in the section, but open and furnished with a zinc cylinder within a bladder attached to a cast zinc ring, from which results a double battery; in which one of the zinc cylinders has both of its surfaces opposed to copper; and I find that on uniting the two zinc cylinders in the manner of a calorimoter, a greater physiological effect is produced (with the aid of an electro-magnet) than I have been able to obtain by any other disposition of the same elements; such as using their zinc elements, either jointly or in succession, with copper when placed as two distinct batteries. This fact although somewhat contrary to that which we might *a priori* expect, is very easily verified by experiment. Of course the electro-magnet's primary coil should for this experiment be made of very thick wire, capable of transmitting readily the full quantity of the electric fluid yielded by this double battery.

I have the honour to be,
Dear Sir,
Your obedient and obliged servant
E. S. CLARKE.

---

## XXXVII. *Experiments upon the ignition of pure Phosphorus by frictional electricity. By* Dr. Boettger.

(Translated from the German.)

In several manuals of natural philosophy and of chemistry, we find it stated, without at all alluding to the method of

conducting the experiments or the precautions to be therein observed, that phosphorus may be very easily inflamed by electricity. Lichtenberg, indeed, observes in a passage in his miscellaneous works (Vol. IX., Gottingen, 1806), "it is deserving of a trial also whether phosphorus would not become ignited at points whence a stream is issuing, on a semiconductor being inserted between them. I am, however, inclined to think that the other portion must also be a perfect conductor."

I have occasionally succeeded in igniting phosphorus by electricity; sometimes, however, I failed in my attempts to do so, without at first being able to assign any reason for my want of success. And as I could not, as above observed, meet with any explanation of this phenomenon I resolved to make some experiments upon the subject myself. The following are the results I obtained, and possibly they may be the means of directing the attention of other observers to this subject.

In all my experiments I made use of purified phosphorus formed into grains about as large as a pin's head. The Leyden jar I employed had a coating of a square foot, and I had a discharging rod with a glass handle the arms of which terminated in extremely fine points on to which small brass knobs could be screwed. I moreover used a chain, one end of which was connected with the outside coating of the jar, the other end terminated in a fine steel needle passing through a glass tube of narrow bore; this needle also allowing a knob to be screwed on to it. I ought to add that to ensure success in these experiments care must be taken not to employ too brittle phosphorus, and above all that it has no cracks in it. It is necessary that it should be duly dried with soft blotting paper just before it is used, and the fine point of the discharging rod or of the steel needle traversing the glass tube, on to which the phosphorus is stuck, should not be inserted much above half way through it.

When I placed the phosphorus upon the steel needle which was directly connected with the external coating of the jar, charged either with positive or negative electricity, and discharged the charge upon it by means of the discharging rod with its knobs, so that the + or — electricity was forced to pass from the knob of the jar on to the knob of the discharging rod, its other knob being held close to the point on which the phosphorus was placed, the latter was *not ignited*. It was, however, now and then broken to pieces and scattered about by the shock. On so modifying the experiment as to present the point of the discharging rod to the phosphorus in place of

its knob, there likewise was *no ignition.* Nor did any ensue either when the phosphorus was stuck on the point of the discharging rod instead of the point of the steel needle, the former of these points being at the same time brought close to the latter terminating either with its knob or otherwise. Hence there was no ignition of the phosphorus when the electric spark is forced to pass through two openings in the circuit, as is shown at *a, b, c, d,* fig. 55, Plate V. In this figure *a, b, c, d,* are the jars charged either with + or — Electricity; *e, e, e, e,** are the discharging rods terminating in knobs or points; *f, f, f, f,* are the steel needles connected with the external coating of the jar and passing through glass tubes; *i, i, i, i,* are the pieces of phosphorus.

I. If on discharging the jar their was *no* interruption between one knob of the discharging rod and the knob in connexion with the internal coating of the jar, in other words if I laid the knob of the discharging rod *directly* upon the knob of the jar and then pursued precisely the course stated above, the phosphorus became ignited *every time* when the jar was charged with *positive* electricity, the phosphorus being stuck on to the needle connected with the outer coating, and either the knob or the point of the discharging rod being suddenly brought near it, as shown at jars *a* and *b* of fig. 56. On the contrary, if I stuck it on to the point of the discharging rod and brought the latter close, either to the point or the knob of the steel needle, as shown at jars *c* and *d* in fig. 56, it was *never* ignited when the jar was *full charged.* When the jar, however, contained merely a very weak charge, a single spark of an inch long for instance, drawn from the prime conductor, the phosphorus was ignited *every time.*

II. On charging the jar with *negative* electricity precisely the reverse was obtained. That is to say, the phosphorus, when stuck on to the steel needle connected with the outer coating, as seen at *a* and *b* of fig. 56 was *not ignited* by a *high charge,* but it took fire *every time* when stuck on to the point of the discharging rod as shown at *c* and *d* in fig. 56 or when the jar, as in the above experiment, was charged with but a single weak spark. It cannot but be considered a remarkable phenomenon that the phosphorus *never* became ignited when treated as described in I, but on the contrary that it *always* was so with a *full* charge of *positive* electricity when treated as in II, the phosphorus being in connexion with the *negative coating.* And that with a *negative charge* if the phosphorus was in connexion with the *negatively charged knob of the*

* The glass handles are not shown in the figures.

*jar,* it was always inflamed, while it *never* was in the other case, though it was so if the jar was charged with simply a very weak spark taken from the conductor of the machine. It is desirable that some person who has more spare time than I can command, should follow up experimentally, what is here mentioned in a general way only.

---

## XXXVIII. *On the Propagation of Electrical Currents through Liquids.* *By* M. Charles Matteucci.*

(*Continued from page* 126.)

### CHAPTER II.

*On the influence of the quantity of the liquid on the intensity of the current transmitted to it.*

I shall notice in a few words the extent of our knowledge on this point of the science of electric currents. In a work by M. Marianini we find it asserted, in a positive manner, that the *velocity* of the current is much weakened by the thickness of the liquid bed it has to traverse: this loss is nearly in proportion to the increase of the thickness, whatever may be the nature and energy of the voltaic pairs. In fact, this influence of the thickness is as much less as the liquid is endowed with a greater conductibility; these conclusions have since been confirmed by M. Bigeon. Having applied myself to this subject, I have established these results by a more exact method, and have added, that this weakening is generally made in a more rapid proportion than that in which we vary the increase of thickness. It is easily perceived that this phenomena has been as yet very imperfectly studied, and that there still remains the same work to be done on this point, as we have already had on the nature of the liquid. It has been our endeavour to fill up this vacancy.

I shall commence by showing the results obtained from a trial of the influence of the length of the liquid bed. I had for this purpose a rectangular canal of varnished wood, one centimetre wide, and two and a-half deep. The electrodes were platinum, three millimetres wide, and immersed in the liquid to the depth of fifteen millimetres. The two electrodes are fixed to two pieces of brass, in connexion with two wooden supports, which move along the canal. The first experiments that I shall mention relate to the study of the enfeebling of

* Translated by Mr. J. H. Lang.

the current by a liquid bed of a greater or less length. The pile is that with columns, which I have already described in the preceding chapter. The liquid of the pile was pump water; that of the canal was composed of 6 ounces of distilled water, and 20 grains of sulphate of magnesia. The following is the first table;

| Length of the liquid bed. | Number of pairs. | | | | | |
|---|---|---|---|---|---|---|
| | 10 | 20 | 40 | 60 | 80 | 100 |
| 1m | 1° int. | 1° ex. int. | 3° int. | 3° int. | 4° int. | 4° int. |
| 0 ,50 | 1 ,5 id. | 2 id. | 4 id. | 5 ex. id. | 5 id. | 5 id. |
| 0 ,30 | 2 id. | 3 id | 5 id. | 6 id. | 5 id. | 6 id. |
| 0 ,10 | 3 id. | 4 ,45 id. | 7 8 id. | 8 10 id. | 7 8 id. | 7 8 id. |
| 0 ,025 | 4 id. | 7 8 id. | 9 12 id. | 10 13 id. | 7 ex. 8 id. | 7 8 id. |

With the same pile and every other part of the apparatus similarly disposed, having only changed the liquid reophorus, rendering it by some drops of nitro-sulphuric acid a better conductor, I obtained the following results:

*2d. Table.*

| Length of the liquid bed. | Number of pairs. | | | | | |
|---|---|---|---|---|---|---|
| | 10 | 20 | 40 | 60 | 80 | 100 |
| 1m | „ int. | „ int. | „ int. | 5° ex. int. | 5° ex. int. | 7° 8 int. |
| 0 ,50 | „ id. | 6° id. | 6° id. | 6 id. | 5 id. | 7 8 id. |
| 0 ,30 | „ id. | 6 id. | 6 id. | 6 id | 6 id. | 7 8 id. |
| 0 ,10 | 8° 10 id. | 6 id. | 6 id | 6 ex. id. | 6 id. | 7 8 id. |
| 0 ,025 | 9 12 id | 10 13 id. | 9 12 id. | 10 13 id. | 9 12 id | 8 10 id. |

I might also add many more experiments, which would only confirm those we have already mentioned.

The conclusions are evident.

1. The intensity of the current, propagated through a liquid, increases generally by the diminution of the length of the liquid bed that it has to traverse; and this increase varies in a greater respect proportionally to the diminution of the length of the liquid bed;

2. When the pile has a very feeble power of production and propagation, we find, in varying the number of pairs, that the increase of the intensity by the shortening of the liquid bed is not in the same proportion;

3. This increase is as much less as the liquid reophorus is a better conductor, and the number of pairs greater;

4. Hence, by increasing the number of plates, and with a good conducting liquid reophorus, we may obtain a maximum intensity, to which the influence of the length of the liquid bed is nothing. We arrive at the limit sooner, in proportion as the liquid reophorus is a better conductor.

These conclusions are very different when we vary the force of the pile. I charged the same pile with a nitro-sulphuric solution. The liquid reophorous still remaining the same as in the first table.

*3d. Table.*

| Length of the liquid bed. | Number of pairs. | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 10 | | | 20 | | | 30 | | | 40 | | |
| 1m | 2° | | int. | 5° | | id. | 5° | | int. | 7° | 8 | int. |
| 0 ,50 | 3 | | id. | 6 | | id. | 11 | 16 | id. | 15 | 26 | id. |
| 0 ,30 | 5 | | id. | 11 | exc.: 17 | int. | 16 | 28 | id. | 22 | ,, | id, |
| 0 ,10 | 10 | 13 | id. | 22 | ,, | id. | 35 | ,, | id. | 50 | ,, | id. |
| 0 ,025 | 24 | ,, | id. | 55 | ,, | id. | 70 | ,, | id. | 80 | ,, | id. |

We evidently conclude, from this third table, that, when the pile has a very great power of production and propagation, the increase of intensity produced by the diminution of the length of the liquid bed, is in as much greater a proportion as the pile is composed of a greater number of pairs.

This law is still more verified, when the current is made to traverse a better conducting liquid, such as that of the second table.

*4th. Table.*

| Length of the liquid bed. | Number of pairs. | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | 10 | | | 20 | | | 30 | | |
| 0m ,50 | 6° | | int. | 14° | 24 | int. | 20 | ,, | int. |
| 0 ,30 | 10 | 13 | id | 20 | ,, | id. | 38 | ,, | id. |
| 0 ,10 | 18 | 36 | id. | 40 | ,, | id. | 60 | ,, | id. |

We will now see what effect is produced by the increase of the surface of the pairs. The liquid of the pile, pump water, that of the trough, distilled water; its simple surface was 45.

*5th. Table.*

| Length of the liquid bed. | Extent of the surface of voltaic pairs. | | |
|---|---|---|---|
| | Simple surface. | Double surface. | Triple surface. |
| 1m | 2° int. | 2° int. | 2° int. |
| 0 ,50 | 3 id. | 3 id. | 3 id. |
| 0 ,30 | 4 id. | 4 id. | 4 id. |
| 0 ,10 | 6 exc. 7 id. | 6 exc. 7 id. | 7 8 id. |
| 0 ,025 | 7 exc. 8 id. | 8 10 id. | 10 13 id. |

With the same pile, and a better conducting solution in the trough, (that of the first table) we have the following results.

*6th. Table.*

| Length of the liquid bed. | Extent of the surface of voltaic pairs. | | |
|---|---|---|---|
| | Simple surface. | Double surface. | Triple surface. |
| 1m | 4° int. | 4° int. | 4° int. |
| 0 ,50 | 5 id. | 5 id. | 7 8 id. |
| 0 ,30 | 7 8 id. | 7 8 id. | 8 10 id. |
| 0 ,10 | 8 10 id. | 10 18 id. | 11 16 id. |
| 0 ,025 | 10 13 id. | 14 24 id. | 15 26 id. |

Thus it may be seen that, in proportion to the intensity of the current, which we vary with the extent of the surfaces of the pairs, the effect produced varies also by the diminution of the length of the liquid bed, and that in as much greater a proportion as the surface of the pairs is more extended and the liquid reophorus a better conductor. I have also tried the same experiments using a more active and better conducting liquid in the pile. I shall exhibit two or more tables, obtained by employing distilled water in the one for a liquid reophorus, and in the other slightly acidulated water. The liquid of the pile is a nitro-sulphuric solution. The simple surface is 67cc 5.

*7th. Table.*

| Length of the liquid bed. | Simple surface. | Double surface. |
|---|---|---|
| 1m | scarcely any traces. | scarcely any traces. |
| 0 ,50 | 4° int. | 4° int. |
| 0 ,30 | 7 8 id. | 8 10 id. |
| 0 ,10 | 13 23 id. | 22 „ id. |
| 0 .025 | 28 „ id. | 45 „ id. |

*8th. Table.*

| Length of the liquid bed. | Simple surface. | Double surface. |
|---|---|---|
| 1m | 7° 8 int. | 8° 10 int. |
| 0 ,50 | 14 24 id. | 16 27 id. |
| 0 ,30 | 23 exc. „ id. | 25 „ id. |
| 0 ,10 | 44 „ id. | 48 „ id. |
| 0 ,025 | 70 „ id. | 80 „ id. |

When the force of production and propagation is great in the pile, we see the increase of intensity, produced by the diminution of the length of the liquid bed, take place in as much greater a proportion as this force is greater, and the liquid reophorus a better conductor.

After having shown the laws by which the effect produced by the diminution of the length of the liquid bed on the intensity of the current which is transmitted to it varies, and that in regard to the force of the pile, I shall examine this same influence produced by the variations of the total volume of the liquid.

Hitherto I have only varied one of the dimensions of the volume, the length, keeping the other two constant; I shall now vary the latter, keeping the length constant. I employed, for these researches, rectangular boxes of varnished wood. I had them of all dimensions. The platina electrodes are discovered by a constant quantity of their surface, and are immersed in the liquid so as to touch the bottom of the box.

We shall see by the results of this examination, that the position of the electrodes, is not indifferent to the transmissibility of the electric current. I commenced by considering the effect of a variable liquid volume, causing it to be traversed by a current produced from a pile, charged with a very feeble conducting liquid. I have collected in different tables, comparable results. The liquid of the pile and also that of the reophorus was distilled water. All my boxes had the constant length of $0^m,20$: the electrodes were platina and presented to the liquids a surface of one centimetre high, and three millimetres wide. In the first table I shall show, this liquid was $0^m,01$ deep.

1*st. Table.*

| Number of pairs. | Width of the liquid bed. | | | |
|---|---|---|---|---|
| | $0^m,01$ | $0^m,04$ | $0^m,08$ | $0^m,2$ |
| 10 | 5 int. | 10 int. | 9 int. | 8 int. |
| 20 | 8 | 11 | 10 | 10 |
| 40 | 12,50 | 21 | 18 | 15 |
| 60 | 15 | 22 | 18 | 16,50 |
| 80 | 18 | 22 | 22 | 19,50 |
| 100 | 21 | 22 | 22 | 21 |

In another experiment, comparing liquid beds only, one of $0^m$ 04 the other of $0^m$ 20 wide, keeping a similar disposition in the rest of the apparatus, I obtained the following results:

*2d. Table.*

| Number of pairs. | Width of the liquid bed. | |
|---|---|---|
| | 0m,04 | 0m,20 |
| 10 | 15 int | 6 int. |
| 20 | | 12 |
| 40 | 23 | 16,50 |
| 60 | 27 | 19,50 |
| 80 | 27 | 21 |
| 100 | 28 | 23 |

I think it useless to mention any more experiments; the results are too evident, and agree very well with the facts we have just established.

Increasing the width of a liquid bed, its depth and height being constant, the intensity of a current made to traverse it, increases to a certain limit of this width: for this the pile must be feeble; beyond this limit the intensity of the current begins to diminish; this limit is attained sooner with feeble piles, and as much sooner as they are composed of a greater number of pairs.

If instead of a very feeble conducting reophorus, we use a better conducting liquid, the results will be as follows. The disposition in the pile and apparatus was alike, the only difference was in the liquid reophorus; in the third table, the liquid reophorus was distilled water; in the fourth, pump water slightly acidulated with sulphuric acid.

*3d. Table.**

| Number of pairs. | Width of the liquid bed. | | | | |
|---|---|---|---|---|---|
| | 0m,01 | 0m,02 | 0m,04 | 0m,08 | 0m,020 |
| 20 | 8 int. | | 8 int. | 9 int. | 8 int. |
| 40 | 14 | | 14 | 14 | 14 |
| 100 | 18 | | 18 | 21 | 19 |

* In all the tables I am about to describe, I have used a more sensitive galvanometer, whose table of intensities has been determined as far as 40°. In the preceding chapters I used a less sensitive one, the intensities of which had only been determined as far as 18°.

*4th. Table.*

| Number of pairs. | Width of the liquid bed. | | | | |
|---|---|---|---|---|---|
| | 0m,01 | 0m,02 | 0m,04 | 0m,08 | 0m,30 |
| 20 | 18 int. | 21 int. | 22,50 int. | 22,50 int. | 22 int. |
| 40 | 23,50 | 23,50 | 23,50 | 23 | 22 |
| 60 | 23,50 | 23,50 | 27 | 26 | 23,50 |
| 100 | 27 | 27 | 27 | 27 | 27 |

We may now conclude that the intensity of the current transmitted by a good liquid conductor, if the pile be feeble, increases by the augmentation of the width of the liquid bed; but the width attains the limit of its influence as much sooner as the number of pairs in the pile is greater, and the conductibility of the liquid reophorus better.

So far, I have only changed the width and length of the liquid bed; I have now to examine the influence of the height of the liquid. I shall commence by showing the results I obtained with very feeble piles. The apparatus was the same in all its parts, only the liquid was employed at different heights: these heights varied from $0^m$,01 to $0^m$,04. The following is the general result. "The volume-*limit*, that is to say, beyond which the increase of the intensity, by the augmentation of the volume itself, stops, is attained as much sooner as the pairs are more numerous, and the conductibility of the liquid reophorus better. Varying the force of production and propagation in the pile, the results vary." I have combined in three tables a certain number of experiments, in which the results are observed by the difference of the height: I think it useless to mention others, as they only confirm those I am about to show, neither do I think it necessary to describe again the pile, electrodes, and boxes, as I have made no alteration in them. The liquid of the pile is composed of one thousand parts of distilled water, six of nitric acid, and two of sulphuric acid in volume: the liquid reophorus was distilled water.

*1st. Table.*

| Number of pairs. | Width of the bed. | | | | Height of the bed. |
|---|---|---|---|---|---|
| | 0m,01 | 0m,04 | 0m,08 | 0m,20 | |
| 10 | 5 int. | 6 int. | 5 int. | 5 int. | 0m,01 |
| 20 | 12 | 15 | 15 | 12 | |
| 30 | 19 | 30 | 30 | 29 | |
| 40 | 29 | 40 | 40 | 40 | |
| 50 | 45 | 55 | 55 | 55 | |

*2d. Table.*

| | | | | | |
|---|---|---|---|---|---|
| 20 | 18 int. | 21 int. | 22,45 int. | 31 int. | |
| 30 | 33 | 36 | 40 | 49 | 0m,01 |
| 50 | 64 | 75 | (42°)? | (52°)? | |

*3d. Table.*

| | | | | | |
|---|---|---|---|---|---|
| 30 | 38 int. | 49 int. | (42°)? | | 0m,03 |
| 50 | (50°)? | (52°)? | (68°)? | | |

These experiments are sufficient for us to draw the following conclusions.

1. When the productive and propagating force in the pile is great, either by the nature of the liquid or the extent of the pairs, the intensity of the current, propagated by different liquid volumes, increases in proportion to these volumes, and in as much more elevated a ratio as the pairs are more numerous and the liquid reophorus a better conductor.

2. We have seen that with feeble piles, we attain the volume-*limit*, beyond which the intensity of the current becomes more feeble. These volume-limits are attained sooner with a pile of a greater number of pairs, and a good conducting liquid reophorus. When the pile is strong, this volume-limit terminates by disappearing, and that in proportion to the force of the pile, the number of pairs, and the conductibility of the liquid reophorus.

3. This volume-limit is also modified by the height of the liquid; the same volume of a certain liquid, which is limited with a certain pile, when the height of the liquid is one centimetre, is no longer limited, every thing else remaining the same, when the liquid is three centimetres high.

4. This augmentation of the intensity in the current, produced by the increase of the liquid volume from which it is propagated, is as much stronger with the same pile and equal volumes, as the height of the liquid is greater, and this influence of the height increases in proportion to the volume. I shall give a table in which these comparisons are made.

5. The advantage derived from the greater height of the liquid, with equal volumes, is so great, that with piles which differ from 20, 30, to 40 pairs, and a difference in the height of the liquid from one to three centimetres, we obtain equal and even more intense currents, from a pile of a less number of pairs whose current traverses a higher liquid.

6. This advantage produced by the greater height of the liquid is such that with piles made of the same number of pairs, we have, with a volume represented by forty, two centimetres high, a stronger current than what we have from a volume represented by eighty, the height of the liquid being only one centimetre.

The following are the tables which I mentioned in the fourth conclusion.

| | | | | | | Height of the Liquid. |
|---|---|---|---|---|---|---|
| Volumes | 20 | 40 | 80 | 160 | 400 | 0m,01 |
| Intensity | 5 | 8 | 10 | 12·50 | 18 | |
| Volumes | 40 | 80 | 160 | 320 | 800 | 0m.02 |
| Intensity | 8 | 14 | 23·50 | 29 | 35 | |
| Volumes | 80 | 160 | 320 | 640 | 1600 | 0m.04 |
| Intensity | | 27 | 35 | 52 | 75 | |

It is only necessary to compare the intensities corresponding to equal volumes in the three series, to obtain the influence of the volume. Thus for the same volume 80, we find 10 in the first and 14 in the second series. For volume 160 we find 12·50, 23·50, and 27, and so on.

The last table that I shall give still better establishes the influence of the height of the liquid.

| Number of pairs. | Volumes. | | | Height of the liquid. |
|---|---|---|---|---|
| | 80 | 160 | 400 | |
| 20 | 14 int. | 14 int. | 12 int. | 0m,01 |
| 30 | 24 | 23 | 23 | |
| 40 | 30 | 30 | 30 | |
| 50 | 36 | 36 | 34 | |

| Number of pairs. | Volumes. | | | | Height of the liquid. |
|---|---|---|---|---|---|
| | 40 | 160 | 320 | 800 | |
| 20 | 16 int. | 18 int. | 19 int. | 25 int. | 0m,02 |
| 30 | 26 | 28 | 30 | 34 | |
| 50 | 40 | 40 | 42 | 52 | |

| Number of pairs. | Volumes. | | | Height of the liquid. |
|---|---|---|---|---|
| | 240 | 480 | 1200 | |
| 30 | 30 int. | 35 int. | 42 int. | 0m,03 |
| 50 | 50 | 52 | 68 | |

The fifth and sixth consequences are easily confirmed by the simple inspection of this last table.

*(To be continued.)*

## XXXIX. *British Association for the Promotion of Science. Eighth Meeting, at Newcastle upon Tyne: in August*, 1838.

*On the possibility of obtaining crystalline metals between the poles of a voltaic battery. By* Dr. Bird.

The author first alluding to the probability of electric currents influencing the formation of mineral veins, observed that in consequence of certain sources of fallacy the results of his experiments which he brought forward at the Liverpool meeting were not very satisfactory. He admitted the interesting character of the experiments of Mr. Fox, and some other investigators, but held them unsatisfactory in point of correct evidence. The author described an apparatus which had been made by Mr. Sandall, of St. Thomas's Hospital. With this apparatus, which consisted of a jar, divided into two compartments, by a partition of plaster of Paris, and a metallic pair of copper and zinc, the former immersed in a solution of sulphate of copper in one compartment; and the latter into water in the other; he found results, which he thought were conclusive in favour of the reduction of a metal between the poles of a battery. At the end of a month Dr. Bird found that the cuperous solution had become colourless: and on taking out the metals he observed that very little copper had been deposited on the copper plate, but the side of the plaster partition *next* to it, was covered with precipitated copper, which the author described as being formed in a *nodular* or stalagmitic form. On breaking the plaster partition, depositions of liberated copper particles, are said to have been found to be disseminated through it in every direction: and thought to resemble the veins of copper found in the interior of the earth.

It must be well known to every one experimenting with cuperous solutions as the exciting solution in a voltaic arrangement: having unamalgamated zinc, covered with either cloth or paper, for one of the metals, that the particles of copper liberated from the solution will be lodged in the crevices of the folds of the paper or cloth, in greater abundance than in any other part of the arrangement. When cloth is employed for covering the zinc, the fibres and threads form a good hold for the particles of copper to cling to: and a thick covering of the metal is always the result of a prolonged experiment. With a *couronne des tasses* of eight pairs, the cloth will receive a tolerably good casing in a quarter of an hour. Even in five minutes' action, the cuperous casing is sufficiently perceptible. Edit.

*On the influence of voltaic combinations on chemical Action.* *By* Dr. Andrews.

In dilute sulphuric acid composed of one atom of the dry acid and eight atoms of water, the solution of distilled zinc is permanently accelerated, by connecting it with a plate of platina, immersed in the same liquid so as to form a voltaic combination. In acid containing seven atoms of water, the usual action is at first increased, and afterwards somewhat diminished by contact with platina. But when zinc is heated in acid, containing less than this quantity of water, the connexion with platina transfers the evolution of gas, from the surface of the positive to that of the negative metal, and at the same time diminishes its quantity, and, consequently, retards the rate of solution of the zinc. The formation of a galvanic circle exerts, therefore, a reverse effect on the solution of zinc in sulphuric acid, containing more or less than seven atoms of water. The principal circumstances which influence these results are, the adhesion of the hydrogen gas to the surface of the zinc: the formation of sulphate of zinc which is greatly facilitated by the presence of seven atoms of water in union with each atom of acid (that being the number of atoms of water of crystallization contained in it): and lastly, the proper action of the voltaic circle, which tends to diminish the solution of zinc. In dilute acid, the first circumstance retards the action on the zinc alone, and the second facilitates its solution: then the platina surface enables the hydrogen to escape. But in the stronger acid, the voltaic association impedes the solution of the zinc, partly from the evolution of gas being transferred to the platina, and thus the saturated liquid being allowed to accumulate around the zinc plate: and partly from the real effect of the galvanic combination. That the proper tendency of a voltaic circle is, to diminish the chemical action of the solution on the electro-positive metal, the author endeavoured to show, from the consideration, that in an ordinary solution, the electricities thus developed have only an indefinitely small portion of liquid to traverse, while in voltaic solution, their reunion can only be effected by passing across a column of variable extent, and composed of an imperfectly conducting substance. And, as the action is greater the nearer the plates are to each other, that action ought to attain a maximum when the distance between the plates vanishes, provided this condition could actually be realized.

*Athenæum.*

*On a horizontal vein of carbonate of zinc resulting from voltaic action. By* ROBERT WERE FOX, ESQ.

Mr. Fox stated, in this communication, that having, on former occasions, announced that he had obtained clay in a completely laminated form, like clay slate, by means of long continued voltaic action: and also, in some instances, vertical veins of oxide of iron &c., alternating with the parallel laminæ: he had now the pleasure of reporting to the geological section, that he had obtained a vein of carbonate of zinc in nearly a horizontal direction by the same agency. The following is a pretty exact description of the voltaic arrangement. A quantity of finely pulverized slate was made into a stiff paste by kneading it with a strong solution of common salt. This clay was next placed on a plate of zinc which rested horizontally on the bottom of an earthenware vessel: forming a horizontal bed of about 1½ inch in thickness. On the upper surface of this bed of clay, rested, also horizontally, a plate of copper, about 1½ inch wide, which was united to the zinc plate by copper wire. The copper plate was covered to some depth by salt water. A vein of carbonate of zinc formed in the bed of pulverized slate. The latter adhered so firmly to the containing earthenware vessel that in taking it out when dry, it was broken into many pieces. Mr. Fox sent one of these pieces to the Association in which a part of the vein is said to be shown; its thickness does not exceed $\frac{1}{30}$ of an inch; and it extended, apparently, in a plate of almost uniform thickness over several square inches, in nearly the middle of the bed.

The author stated that he had detected a little iron in it, and numerous grains of quartz, which were so firmly imbedded in the vein that it would scratch glass. In this instance the voltaic action between the two plates was *perpendicularly* through the bed of slate clay, and there resulted a *horizontal* or flat vein. The common salt kept up a degree of moisture in the apparatus, and the experiment was continued more than eight months: the carbonic acid was supposed to be derived from the atmosphere. It was observed that on all other occasions, the veins as well as the laminæ of the clay were *vertical*, the voltaic current *horizontal*. One of these *vertical* veins consists also of carbonate of zinc formed in the *middle* of a wall of clay having had copper pyrites and zinc on each side of it. When a solution of sulphate of iron was mixed with the clay, or in contact with it, numerous veins of the oxide of

iron occurred in it similarly arranged. Mr. Fox also stated that he had obtained veins of sulphate of zinc with striæ at right angles to their direction, so as precisely to resemble *satin spar* in appearance. The results of his experiments he thought tended to prove that earthy matter when moistened may form voltaic poles, on which mineral matter may be deposited: and, therefore, it would follow, that rocks charged with moisture, may, as he (Mr. Fox) had assumed in the Cornwall Polytechnic Report for 1836, act the same part in the earth, and influence the deposition of minerals from their solutions in fissures on some rocks in preference to others, according to their relative electrical conditions.

From the experiments already noticed, Mr. Fox considers that the *pre-existence* of a fissure is not always an essential condition for the formation of a mineral vein, as, in the instance alluded to, a *horizontal* mechanical fissure cannot well be imagined to have existed previously to the deposition of the carbonate of zinc. It is true, says Mr. Fox, "that the clay, as I have formerly stated, has sometimes exhibited a strongly marked joint or division near the middle and parallel to the laminæ, which resembled a fissure. And here it seems, that the carbonate of zinc was formed into a vein, taking, as it were, the most neutral part of the circuit, and not accumulating at either of the metallic poles, where its constituents would naturally be expected to be formed. The oxides of iron and of copper, the sulphate of zinc, &c., have also shown a tendency to approach the more neutral part of the voltaic circuit; and it is probable that other metallic compounds and neutral salts may do the same. Moreover, the experiments would induce a belief that many veins which coincide with the laminæ of schistose rocks may have formed by an analagous process in the earth, although it is probable that they are usually less regular and extensive than those veins which from crossing the laminæ, and other circumstances, ought rather to be referred to a mechanical origin. In some instances the formation of *fissures* or *cracks* even in the *moist clay*, parallel to the vertical copper and zinc plates has been most clear and decided, but this was most remarkable when several pairs of plates were used in a series: they then appeared even in a few days after the commencement of the experiments."

Professor Lloyd read a paper, entitled, 'Recalculation of the Observations of the Magnetic Dip and intensity in Ireland, with additional Elements.'

Since the publication of the observations on the direction and intensity of the terrestrial magnetic force in Ireland, in

the fifth Report of the Association, many additional data have been obtained, which appear to require a new and more complete discussion. The present communication, therefore, may be regarded as a supplement to the former; and its results have removed (at least to the extent that could be reasonably looked for) some discrepancies under which the former laboured. The additional data, which form the groundwork of the present recalculation are, chiefly, the following:—1 Additional observations of the dip and force at the central stations; 2. More exact values of the latitudes and longitudes of the several stations at which observations have been made; 3. Nearer approximation to the amount of the annual change of dip; 4. Re-determination of the corrections to be applied to some of the dipping needles; 5. Determination of the weights of the several results, deduced on more exact principles. A very considerable series of dip observations having been made by Mr. Lloyd, in Dublin, it occurred to him that they might furnish a tolerable approximation to the amount of the annual decrease of dip, notwithstanding the limited space of time (three years) over which the observations are spread. Let $u$ denote the *probable* dip at a given epoch (the 1st January, 1836); $a$ the *observed* dip at any other time; $n$ the number of effective months in the interval; and $\Delta u$ the monthly decrease, which is assumed (in accordance with the observations of M. Kupffer) to take place during the eight months from May to December inclusive. Assuming then (as we are unacquainted with the law of monthly change) that the decrease takes place *uniformly* throughout these eight months, it is manifest that each of the results will furnish an equation of condition of the form—

$$u+n\Delta u=a;$$

and if we combine these equations by the method of least squares, we obtain the most probable values of $u$ and $\Delta u$,—i.e., the most probable values of the dip at a given epoch, and of its monthly change. In this manner it is found that the annual decrease of dip in Dublin is $2'.38$. The recent and more complete observations of Major Sabine, in London, make the amount of the annual decrease $2'.40$; and the close agreement of the results must be regarded as affording a strong mutual confirmation. The next subject connected with these observations, which Mr. Lloyd desired to bring under the notice of the Section, is the correction to be applied to the results of the several dipping needles. The errors of dipping needles may be ascribed to one or other of the three following sources:—1. The friction of the axle on its supports; 2. The

imperfect curvature of the axle itself; 3. The magnetism of the limb. The errors arising from the first-mentioned cause, are, however, very different in their nature from those due to the two latter. The *positive* and *negative* errors, due to friction, are equally probable; and the effect of the disturbing cause is merely to widen the limits of probable error. The imperfect curvature of the axle, and the magnetism of the limb, however, act very differently. It will be easily understood that either of these sources of error must, *within a moderate range of dip,* affect all the results in the same manner; so that they will all require a correction having the *same sign;* and when the range of dip is very small, the amount of the disturbance will be nearly the same in all, and the correction required will be nearly constant. A remarkable instance of one of these disturbing influences occurred in the present series of observations. Having purposely destroyed the balance in two of his dipping needles, Mr. Lloyd proceeded to use them exclusively in observations of intensity, according to a method, the principles of which have already been laid before the Association. The results thus obtained, were, however, so anomalous, that he was compelled to lay them aside altogether. After some tedious and vain attempts to discover the source of the anomaly, Mr. Lloyd was at length satisfied that the needles were under the influence of some other force, besides the earth's magnetism and gravity; and he concluded that this disturbing force could be no other than magnetism in the dip-circle itself. Trial soon verified this conjecture; and he had the mortification to find that the apparatus which he had been long using was *throughout* magnetic; and that the magnetism was greatest in the graduated limb, the very part in which, from its proximity to the needle, it would operate most powerfully. He had next to consider the painful question,—How far the numerous results obtained with this instrument were vitiated by this newly-discovered source of error? Whether they were entitled to any confidence; and if so, what were the probable limits of error. It is manifest that if the ferruginous matter were *uniformly* distributed throughout the limb, it could produce no disturbance in the position of a needle, which (like the dipping needle) divides the limb symmetrically. It is only by irregularity in its distribution, that the magnetic matter of the limb can operate as a disturbing cause; and then it is manifestly only by the *difference of the attractions* on the two sides of each pole that the needle is actually disturbed. Hence, though the

magnetism of the limb may exert a very sensible action upon a test needle, in a position at right angles to its plane, the effect upon a dipping needle may be comparatively trifling. Mr. Lloyd next proceeded to institute a series of experiments for the purpose of estimating these effects. Their result showed that the anomalies observed were produced by two centres of ferruginous matter in the neighbourhood of the zero points of the limb; but they likewise showed that, in the usual positions of the needle, the disturbance, though sensible, did not vary rapidly in amount; so that, for moderate changes of angle, the direction of the needle might be considered to be altered by a constant quantity. Thus, in the neighbourhood of three of the divisions of 70°, the needle was sensibly deflected, and in such a manner as to diminish the apparent dip: but the deflection did not vary rapidly with the angle; so that, for small changes of dip, the error might be regarded as nearly constant. Defective, therefore, as the apparatus was in this respect, Mr. Lloyd was satisfied that the *differences of dip* obtained with it in Ireland might be relied on within the usual limits of error in good instruments; and that, to obtain the *absolute dip* from the observed results, it was only necessary to apply a positive correction, which may be regarded as *constant* throughout the series. This correction has been re-examined, by comparing the results of this instrument in Dublin with those of an excellent needle made by Gambey, and observed by the method of arbitrary azimuths, so as to eliminate the errors both of axle and limb. The resulting value of the correction is almost identical with that formerly obtained. In combining the results of observation by the method of least squares, so as to deduce the mean position of the isoclinal and isodynamic lines, it becomes necessary to know the relative value, or *weight*, of each determination. The principles on which these were assigned in the former memoir were, in some degree, arbitrary and inexact; and tended consequently to distort the final results.

In the present investigation, use has been made of a beautiful theorem of the calculus of probabilities, demonstrated by M. Poisson. This theorem furnishes the relation between the the *probable error* of the quantity finally sought, and those of quantities actually observed, the former being supposed to be a linear function of the latter; and from the known relation between the probable error and the *weight*, it admits of easy application to a large class of problems, which are of frequent occurrence in investigations like the present. *Athenæum.*

*(To be continued.)*

## XL. *London Electrical Society.*

*Tuesday, August 7th.*—Read, Observations respecting the regular Variations and other Motions of the Magnetic Needle. By Mr. Leithead.

The author considers electricity and magnetism to be two distinct powers, between which no physical analogy has been proved to exist. The variation of the needle he states to be an indirect effect of electrical action depending upon a disturbance or temporary counteraction of the magnetic by the electric power; and observes, that when the magnetic intensity is at its maximum, the electric intensity is at its minimum, and at the same time the air attains its maximum diurnal intensity. Viewed as isolated facts, these phenomena appear as unintelligible as they are curious; but if considered in connexion with each other, the author thinks them of great importance, inasmuch as they appear to afford a glimpse of a new law upon the operation of which the phenomena are dependent. The following is Mr. Leithead's view of the subject:—"That the intensity of the earth's magnetism is temporarily increased and diminished by phenomena depending upon electrical action." The annual change supposed to depend on the position of the sun in reference to the equinoctial and solstitial points, the diurnal variations of magnetic intensity from which we have selected the foregoing elucidation of Mr. Leithead's views, the variations observed during the appearance of aurora, thunder storms, and other phenomena depending upon electrical action, were severally considered in support of his opinion. The paper evinced much care, study, and intelligence.

After a discussion on the foregoing paper, Mr. Maugham communicated his progress in a very important experiment, having for its object the liquefaction of one or both of the gases constituting water. The gases being liberated from water by galvanism, in a strong tube, hermetically sealed, are thus exposed to their own pressure. He was encouraged to hope, that in his future experiments he should be more successful, and he would from time to time bring the future stages of his progress before the society.

*Tuesday, August 21st.*—Read, A Paper by Mr. Alison, On certain Electrical Phenomena observed some years ago,

at different seasons and temperatures, and at elevations varying from 5000 to 11,500 feet.

The author having resided during a period of thirty years in a mountainous and volcanic country, and having devoted much of his attention to volcanoes in the actual sphere of their action, is convinced that many of the phenomena of nature are caused by electricity and magnetism.

He mentions, that on mountains of the second and third classes, relatively to their height, the electric fluid constantly follows the course of the sun, rising with it till it reaches the meridian, and declining with its rays, and remaining nearly motionless during its absence. On the Alps all is tranquil during the night, when nature appears buried in a profound sleep—all is stationary—the clouds accumulate and envelope the mountains, and rest motionless; but as soon as the sun touches the horizon, electricity shows itself with the light of day, disperses the clouds, and directs itself towards the west, increasing its force in proportion to the angle which the sun's rays form with the horizon till vertical in the meridian, when the electric power decreases in proportion as the angles become smaller, till the sun descends below the horizon. At midnight, however, electricity is observed in a slight degree, after which it remains imperceptible till the rising of the sun.

"In mountains which are covered with perpetual snow, a strong electric current is observed from north to south, at an elevation of from 7000 to 8000 feet, which corresponds to the mean temperature of lat. 60° to 70°.

"The aurora borealis shows us that towards that latitude the electric fluid abandons the earth and takes an inverse direction towards the sun, that is from north to south, under an angle of 45°." This may be the cause of the perpetual current of the icy sea between the parallels of 60° and 70°, and ceases in lat. 55° north and 40° south.

Space will not permit us to follow the author in his interesting account of the phenomena exhibited by the volcanoes of Stromboli, Teneriffe, and others, as also in his views on the connexion between electricity and magnetism, &c.

At the conclusion of the evening an electrical eel (*gymnotus electricus,*) taken in the river Amazons, in South America, was exhibited to the members, and excited considerable attention. It is nearly four feet long, and the transverse diameter of the body about three and a half inches. There are two pairs of electrical organs of different sizes, and placed on different sides; the large one occupies the whole of the lateral part of the fish, constituting the thickness of its fore

part, and extending from the abdomen to the narrow end of the tail, where it terminates nearly in a point. The two organs are separated at the upper part by the muscles of the back, at the lower part by the middle partition, and by the air-bag at the middle part. The lesser organ stretches along the lower edge of the fish, and nearly as far as the others, terminating almost insensibly near the end of the tail. Its power has greatly decreased in a cold climate, but if placed in a temperature of 70° to 80°, it would recover its force.

The subject of the electricity of the torpedo has excited considerable attention through the experiments of M. Matteucci, (See Annals of Electricity, vol. 2, p. 290), but the opportunity of experimenting in this country has seldom offered. It has been stated that more specimens of this animal will, through the influence of one of the members of the society, shortly arrive in this country. We hope, therefore, that by the exertions of this infant society, some further light may be thrown on this very interesting branch of the science.

*Tuesday, September 4th.*—A paper by Mr. Clarke, entitled, Experiments in Magnetic Electricity, was read. In previous notices of this society, we have had occasion to comment favorably upon the experimental investigations of individual members, as well as upon their theoretical views, and to express our favorable opinion of their importance. We have, however, always leaned towards experimental researches, and urged a collection of facts in preference to speculative theories. To theorise, it is true, is to cause people to think, but to register experiments is to cause others to sift and test, and thereby establish data for fixed laws. And upon experimental investigation must chiefly depend the reduction of the wonderful phenomena to these laws. The importance of that desideratum requires no advocacy. For many years the attention of electricians has been directed to this point, and the discovery of the connexion of electricity and magnetism has considerably augmented the chances of its attainment. Yet they may possibly be very far from success, and probably because from want of experiments on a large scale. Certain phenomena, constantly observed and invariably within the limits of past investigations, though so microscopic in comparison with the splendour of this mighty agent when exhibited on the grand scale of nature, have been considered sufficiently established to form a sure foundation for the theoretic superstructure: but the least error in the basis of a science must inevitably lead to its overthrow, and the sooner such errors be detected the better. The experiments about to be described, may,

perhaps, be a step towards attaining true principles by increasing and extending the boundaries of our experimental observations. The dimensions of the machine with which these experiments were performed, greatly exceed any other that has yet been placed on record. The magnetic battery being vertical, the armature rotates at its opposite sides. The quantity arrangement being at one side and the intensity one on the other. The result of the first experiments was so opposite to what had been anticipated by the author that the arrangement was supposed to be defective. The quantity armature was furnished with a short coil of thick insulated copper wire, and the intensity armature with 15.375 yards* of fine copper wire. On trying the electro-gasometer with the intensity arrangement, no decomposition took place, although the shock obtained by it was most excruciating, perhaps even dangerous, to a single individual in the circuit. The decomposing power of the quantity arrangement was next tried and one cubic inch of gas obtained in four minutes. This appearing to the author to be a novel fact, it was supposed to be caused by a compound action produced by the rotation of the two armatures. Mr. Clarke therefore determined to arrange the magnets similarly to those of the machines he had been in the habit of constructing. The only difference consisting in the size of the instrument and in the means of communicating motion to the revolving armature, namely, by a crank and treadles similar to the lathe. The compound magnet consists of ten bent steel bars, each four feet long; the whole weighing 156 lbs. Ivory was made use of to retain the wires on the armatures, in lieu of brass plates, which the author supposed gave uncertain results owing to their conducting property. The novel results of the experiments were, first, the great amount of gas obtained by the quantity arrangement, in one instance one cubic inch in one minute and a half, which result confirmed the correctness of the original arrangement. The second was the trifling decomposing effect from the intensity arrangement. The gasometer employed in the experiments was furnished with two slips of platina, one inch in length, and three-eighths of an inch in breadth; but the decomposing power was considerably increased by the substitution of fine pointed wires of platina. The next experiments, briefly alluded to, referred to the different appearance of the spark with various modifications of the armature. With the intensity arrangement a long straggling noiseless spark is obtained, having much resemblance to the spark which passes

* Such was Mr. Clarke's statement of its length.

from a low charge of the prime conductor of an electric machine to a body placed at a short distance. The quantity arrangement gives a spark which not only has the usual stellar form, but is accompanied with a snapping resembling that of a higher charged conductor. Although these distinctions exist between the sparks, they both appear equally luminous. The illustrative experiments were mostly very good. The best sparks, as is usual with these machines, were those from the surface of mercury.

## XLI. MISCELLANEOUS ARTICLES.

*Note on Mr. Murray's defence of Mr. R. W. Fox's theories. By* W. J. HENWOOD, ESQ. *F. G. S., H. M., Assay Master of Tin, Secretary of the Royal Geological Society of Cornwall.*

*To William Sturgeon, Esq., &c. &c.*

My dear Sir,

I have shown (*Annals of Electricity*, vol. I., p. 227.), that, in the experiment, of which the explanation has been so much discussed between Mr. R. W. Fox and myself, the solution which is at first saturated with the sulphate of copper, becomes quickly capable of dissolving more of that salt; and that until such an addition is made to it, the colour at first induced on the copper pyrites remains unaltered, but that after more of the sulphate of copper is introduced it becomes deeper, and so on by successive additions.

Mr. R. W. Fox (*Annals*, vol. II., p. 115.) takes an objection to this experiment because two ores of copper (which I had used *several* years since) were employed; and says it was only when "*zinc* or some other electro positive metals" were employed, that the ore was changed from the state of pyrites to that of sulphuret of copper.

I have made the experiment in that manner, and find that a solution saturated at first, becomes in a short time capable of taking up more sulphate of copper in that case also.

I repeat my enquiry, what produces this capability of dissolving more of the salt, for the solution was *saturated* at first?

You (*Annals*, vol. II., p. 115.) as well as Mr. Murray (*Annals*, vol. III., p. 64.), seem to adopt Mr. R. W. Fox's explanation of the *change of one ore into another*, in preference to that

drawn from M. Becquerel, of the grey crust being *sulphuret deposited in consequence of the decomposition of the sulphate.*

Neither you, Mr. R. W. Fox, nor Mr. Murray have stated why you prefer the new (and as I think more difficult) assumption, in preference to the old and more obvious one; nor can I see one experiment which favours the former. Permit me respectfully to solicit a reason for this preference.

I have said, and I repeat, there are chemical objections to this new explanation; according to all the acknowledged forms of proceeding the *onus probandi* rests with the propounder of a new doctrine. I therefore repeat my call on Mr. R. W. Fox to show the consistency of his views with chemical phenomena. My objections can only properly come after his case is stated; and notwithstanding the adroitness with which he endeavours to shift the *onus* on me, it is not the fair or usual course of proceeding, for the objections to be taken before the novelty is brought forth.

I remain my dear Sir,

Your much obliged and faithful servant,

W. J. HENWOOD.

4, Clarence Street, Penzance,
July, 23, 1838.

P. S. I have repeated the experiment of the action of voltaic electricity in the manner described by Mr. R. W. Fox, and though the apparatus was kept in action several months, there was not the slightest appearance of lamination in the clay interposed.

---

*Answer to Mr. Henwood's Letter.*

My Dear Sir,

In answer to your enquiries respecting my reason for preferring the theory of Mr. Fox to that of M. Becquerel on the subject in question, I have only to say that I have never yet, either publicly or privately, given any opinion whatever respecting either of them. The specimen of copper ore which Mr. Fox sent to me, had every appearance of having a coating of sulphuret to some depth. I showed it to Mr. Bachhoffner, who also considered the exterior to be a sulphuret, and said that he had himself produced similar specimens by repeating Mr. Fox's experiment. As to the coating of sulphuret being a result of conversion from the original bisulphuret, rests entirely on Mr. Fox's statement: for it would be impossible for me or any other person, unacquainted

with the particulars of the experiment, to have a perfect knowledge of the origin of the sulphuret. The confidence which I considered I had a right to repose in Mr. Fox's statement, in connexion with the facts presented by the specimen sent to me by that gentleman, gave no room to doubt of its correctness. Knowing also that another specimen of Mr. Fox's factitious copper ore had previously been chemically examined and found to approach "so nearly to that of the Cornish sulphuret as to warrant the conclusion of its being the same chemical compound:" and that "the soluble salt was found to be a sulphate of copper and iron, which accounts for the iron that the yellow copper ore had lost during its conversion into sulphuret,"* I could not hesitate for a moment to suppose that the transformation had actually taken place. Still, however, as I have before stated, as I was not acquainted with all the particular circumstances attending Mr. Fox's experiment, it would be impossible to form a correct idea of the nature of the process by which the sulphuret was formed. Hence it was that I did not *positively* state that the transformation from the bisulphuret to the sulphuret actually took place. But from all the circumstances connected with my information, I think you will allow that there was no impropriety in stating that the specimen sent to me "*appeared* to be satisfactory evidence of the change which had taken place."†

From this explanation I hope, my dear sir, you will easily perceive that the expression was far from being of that *positive* character which a decided opinion in favour of any theory would have necessarily demanded: and you may rest perfectly assured that it is very far from my intention to enter into any part of the controversy. The subject, however, is so exceedingly interesting, that I have been induced, as you are well aware, to repeat Mr. Fox's experiments: and in order that I might proceed with them in the most favourable manner for arriving at similar results, I solicited the favour of that gentleman to furnish me with similar materials to those he employed. My request being politely granted, the experiments were immediately commenced‡ and are yet in process. When they are completed, the results, whatever they may be, will be submitted to the consideration of the Electrical Society. You will, therefore, excuse me from giving any theoretical views at present; for should I be induced to give a decisive

* London and Edinburgh Phil. Mag. &c. Vol. X, p. 171.
† Annals, Vol. II, p. 115, note.
‡ See Annals, Vol. II. p. 395, 475.

opinion of the theory of the action, it will be formed from the results of my own experiments, and not from any other source of information.

I am,
My Dear Sir,
Yours very truly,
W. STURGEON.

*Westmoreland Cottage,*
*Sept.* 1838.

---

*Aurora Borealis.*

The Aurora Borealis, after a rather long absence from this neighbourhood, has again made its appearance, and displayed its transient beams twice or more times, within the last week. On Sunday evening, the 16th instant, I observed an unusually strong light among the clouds in the northern parts of the heavens: from which I suspected that an aurora had either appeared in an earlier part of the evening, or that it was about to show itself. The light soon became much stronger, probably from the dispersion of the clouds, which had now mostly left the theatre of display; and the aurora was identified by a few vertical dim streamers that occasionally gleamed on the eastern side of the north point of the meridian. The general glare of light became much stronger in a very short time afterwards, and about 9 o'clock, or perhaps a little after, undulatory waves of light rolled upwards in a very magnificent manner. At this time there did not appear any indication of that black segment which is usually seen beneath the brightest part of an aurora; and the light was so strong, and generally diffused in the northern heavens as to prevent ascertaining, with exactness, both the *source* of the waves and also the highest altitude to which they ascended. They seemed to come into existence at about 8° or 10° of altitude, and roll to elevations between 20° and 60°; and some of them might possibly ascend much higher than the latter altitude. The waves stretched horizontally across the meridian, to between 40° and 50° east and west. They had very much the appearance of illuminated vaporous matter, and did not undulate at a very brisk rate. They were densest at their commencement, and became gradually more and more attenuated as they ascended the heavens, and vanished at their terminal margins in the softest diffusions of pale expiring auroral light. The waves were succeeded by a few gentle streamers of considerable length, but without much motion; and mostly displayed on the western side of the meridian. It is very probable that

had there not been such a general glare of light in the northern part of the heavens, many more streamers would have been seen. The sky became quite clear of clouds a short time after the disappearance of the waves; and the central part of the aurora had obviously taken a position nearly in the magnetic meridian, although, prior to the appearance of the waves, the region of strongest light was as obviously on the eastern side of the true meridian.

WILLIAM STURGEON.

*Westmoreland Cottage,*
*Pomeroy Street, Old Kent Road.*
*Sept.* 29, 1838.

---

*Luna Iris.*

This exceedingly rare phenomenon was witnessed on Sunday evening, between 8 and 9 o'Clock, at Charlton in Kent, bearing N.W. by W. stretching across the horizon towards the nadir, obliquely besetting the milky way, at an angle of 45°.*

*Times, Sep.* 20, 1838.

---

Kennington Grammar School,
September, 19, 1838.

Dear Sir,

Having witnessed a display of the Aurora Borealis on the evening of Thursday, the 13th. instant, I have endeavoured to describe it: although, in doing so, I am unable to be minute because of the brief space of time during which I had the opportunity of seeing it.

About 11, P.M., while passing towards London (along the right-hand side of Clapham Common), the moonless evening was observed to be unusually light. I did not at first anticipate the cause; but on a belief that it might be the Aurora, and not being perfectly acquainted with the bearing of the localities, I turned toward the heavens to obtain a view of Ursa Major, as being the first and most prominent constellation that occurred, but it was hidden. Then I discovered that a large arch, or, more properly speaking, segment of a

* This was probably some of the effects of the Aurora Borealis, which was seen on the same evening. Edit.

circle, appeared filled with *white* light, in the northern quarter of the heavens. The greatest height might have been 20° or 30°. From the circumference of this proceeded rays (or belts rather) of *red* light converging toward the zenith.

In ten minutes after first observing it, the phenomena disappeared. This sudden disappearance made me hesitate before sending an account: but hearing that it was visible again on Sunday, and having on that day myself observed it (though not so manifestly), I have written this very imperfect account.

Judging from circumstances, I should imagine that the centre of the luminous space was some few degrees west of north.

I remain,
Dear Sir,
Your obedient Servant,
CHARLES V. WALKER.

To W. Sturgeon, Esq.

---

### *On the appearance of Encke's Comet in the autumn of the present year.*

*Extract from a Letter from Dr. Olbers to Professor Gruithuisen, dated Bremen,* 31*st December,* 1837.

(Translated from the German.)

The heavens do not at present show us much novelty. Comets are become very scarce. Are they so in reality, or does this arise from our not having as yet any worthy successors of Pons, Gambard, Harding, and others, whose search for them was so unceasing and so well repaid?

One comet, however, Encke's, we shall for a certainty see in 1838; on which occasion that little body will present itself to us as beautifully and distinctly as it ever can before reaching its perihelium.

According to a communication for which I am indebted to the kindness of Professor Encke, it will attain its perihelium at noon, on the 19th of December, 1838.

In 1795, when my attention was first drawn to this remarkable body, it was in a similar position on the 21st of December: its apparent path therefore will, in 1838, be nearly the same as on that occasion, but it will have a more northerly and westerly direction.

The comet now too will approach the earth closer than it did then, almost as close indeed as it can, under any circum-

stances, come. It will be visible through a telescope at the beginning of September, near Musca; and during that month and the first half of October it will pass up between the Triangle and Medusa's Head, and between Andromeda and Perseus. It will then proceed with increased velocity right across Cassiopeia and the shoulders of Cepheus; and from thence it will, in November, at first rapidly and by degrees slower, descend between Draco, Cygnus, and Lyra, to Hercules, whose left side it will pass along, and then proceed across his neck and club. Finally it will direct itself through the west hand of Ophiuchus, and then towards his west leg; its motion having now become direct again, will be lost to us in that neighbourhood, in the early part of December, in the rays of the sun.

Towards the end of October, and in the first half of November, it will, in all probability, be distinctly visible to the naked eye.

---

## *Recipe for an Alloy in imitation of Gold.*

An alloy is made in Germany, (Munich) of zinc and copper called *Chrysorin*, on account of its perfect resemblance to gold of twenty carats. This quality depends on its containing the exact proportion of fifty-one parts of zinc, to one hundred parts of copper; for if, by a heat too powerful or too long continued, a portion of the zinc becomes volatilised, nothing but common brass is produced, without lustre, containing fifty parts of zinc and one hundred of copper.

The greatest precautions, therefore, are requisite in the fusion of the two metals. They begin by putting into the bottom of the crucible one-third of the requisite quantity of zinc, and over it all the copper which is covered with a vitreous flux. This is heated in an air furnace until the copper is well melted, which is known by its assuming a mirror-like surface under the flux. The rest of the zinc is then added in small pieces.

*Jour. de Connais. Usuelles.*

*Jour. Frank. Ins.*

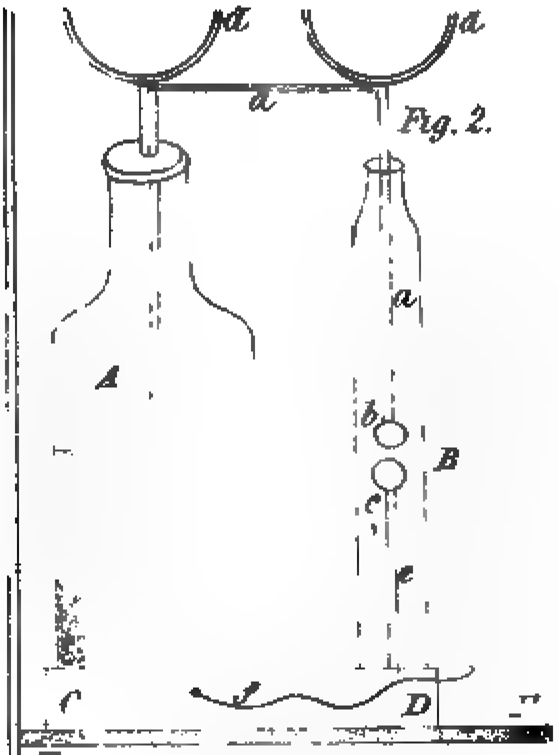



Fig. 2.

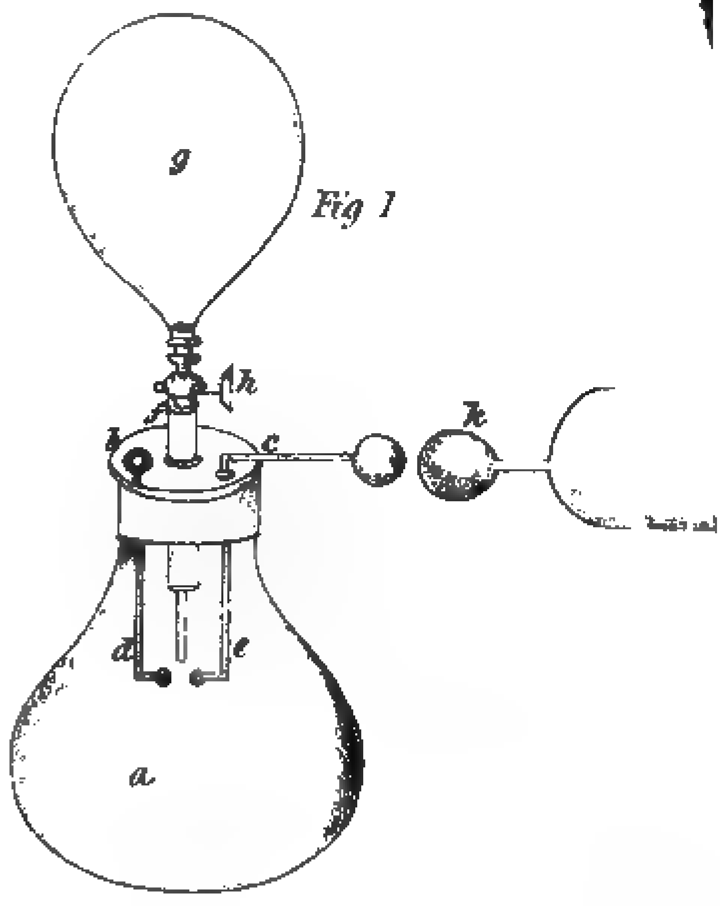



Fig 1

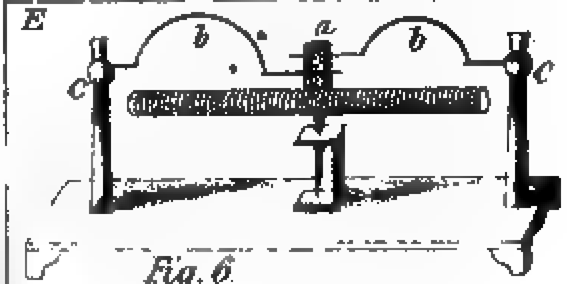



Fig. 6

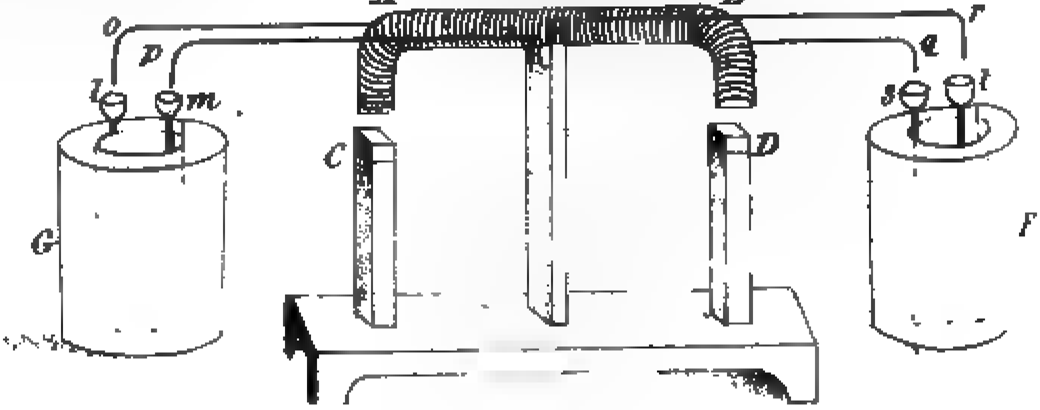



Fig. 4

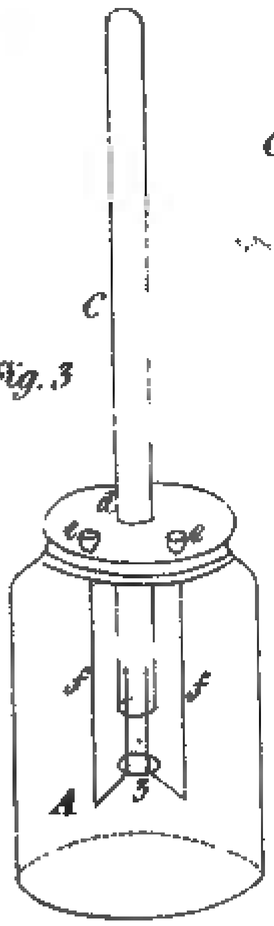



Fig. 3

*Fig. 1*

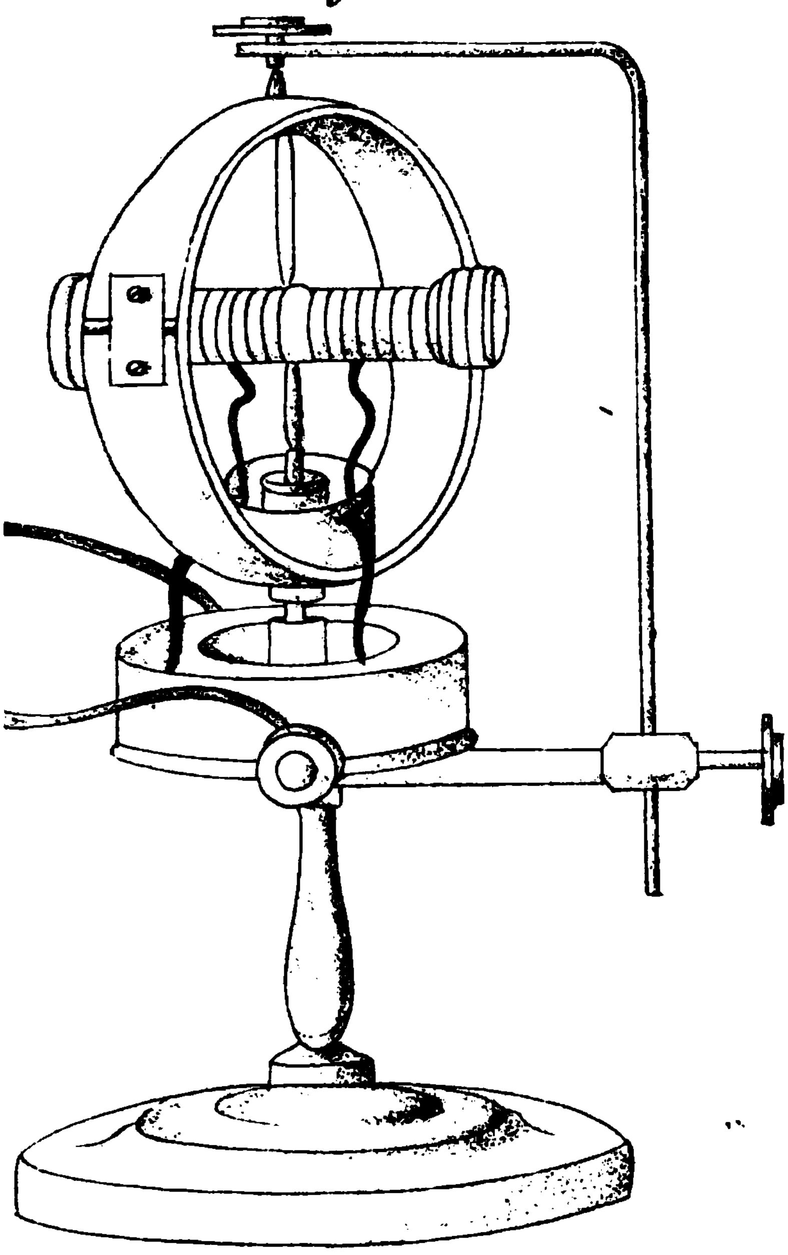


*Fig. 2.*

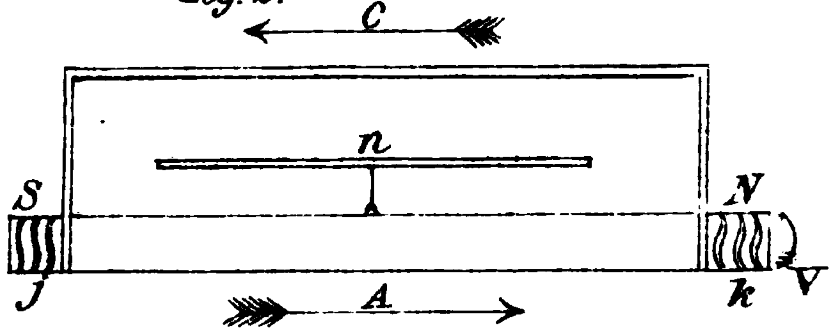



*Fig. 3.*

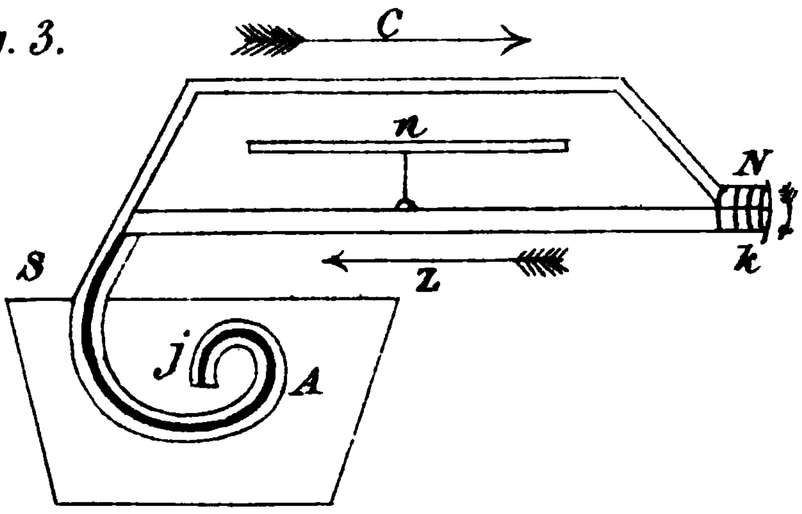



*Fig. 4.*

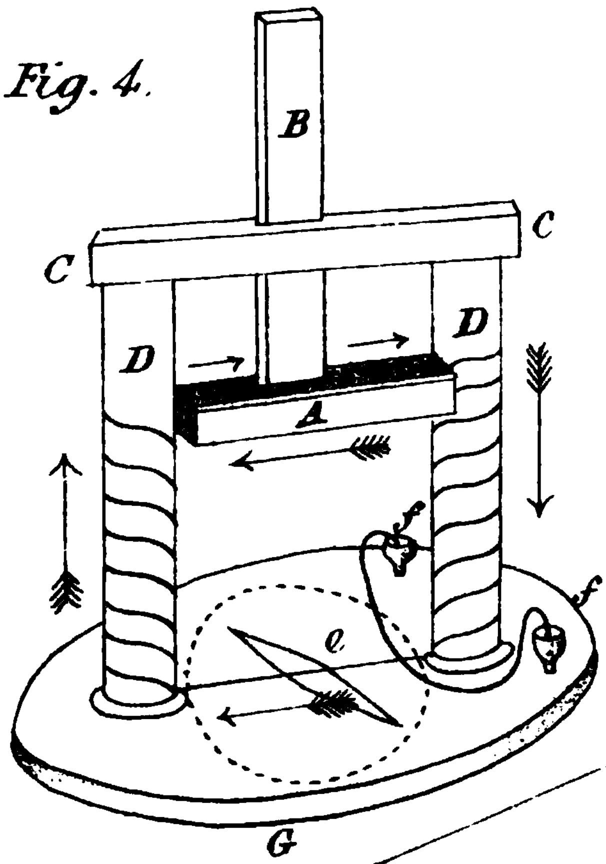
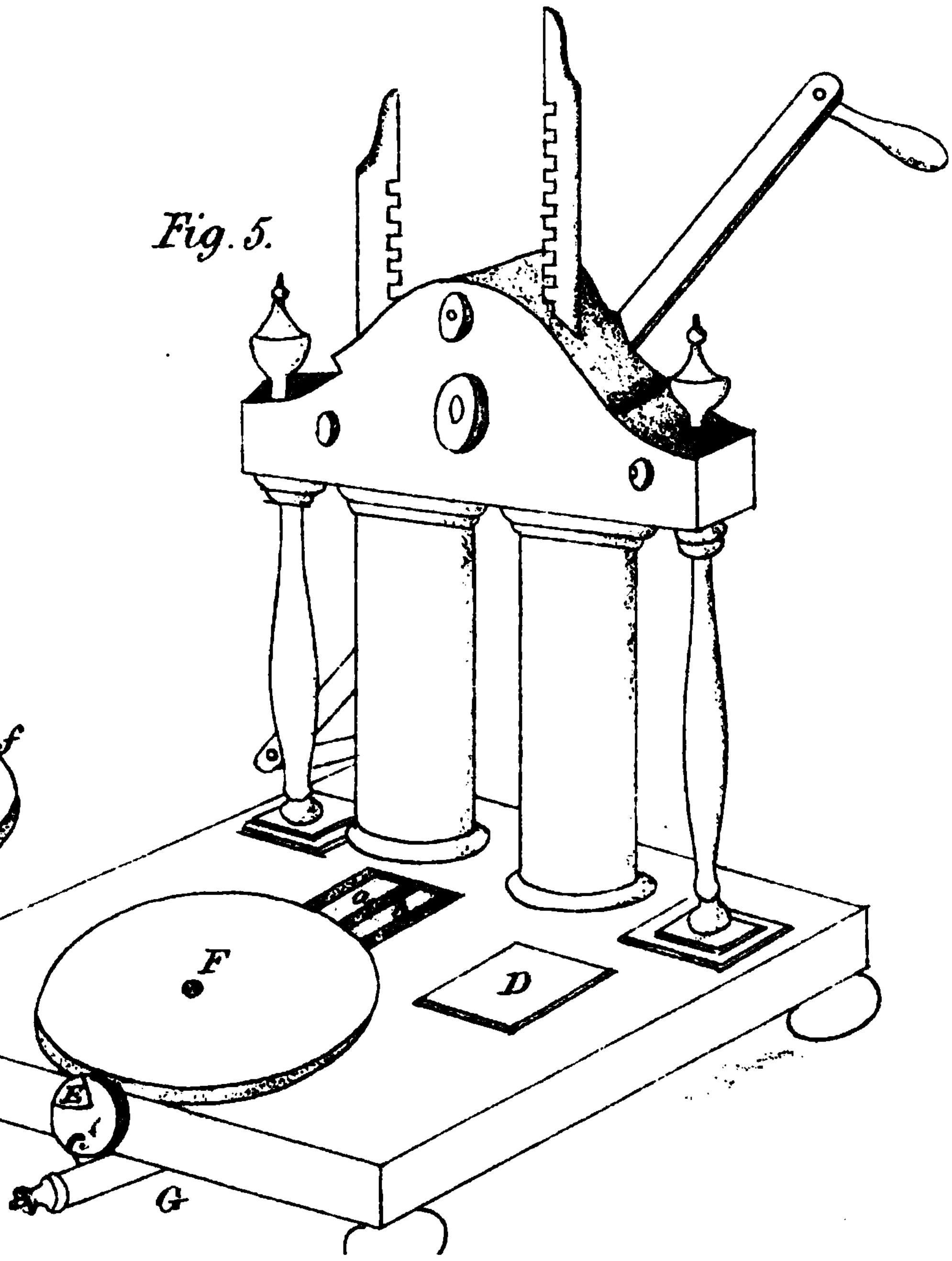



*Fig. 5.*



THE ANNALS
OF
*ELECTRICITY, MAGNETISM,*
*AND CHEMISTRY;*
AND
**Guardian of Experimental Science.**

*JANUARY*, 1839.

## XLII. *Description of the Hydroplasson, a portable, simple, and perfectly secure self-acting Instrument for exhibiting the composition of water from the combustion of its gaseous constituents in a state of previous admixture. By* W. F. Weeks, Esq., *Lecturer on Philosophical and Operative Chemistry, &c. &c., Sandwich.*

The Annals of Chemical philosophy cannot, perhaps, furnish a more signal and brilliant triumph than that afforded by the synthesis or composition of water from two perfectly invisible aeriform fluids, and, though frequent repetition of this grand experiment (the results of which to the uninformed beholder appeared little short of those marvels attributed to the wand of the pretended magician of ancient days), has, by abstracting the novelty, in some degree dissolved the charm, and substituted a more praiseworthy excitement to enquire into cause and effect; the exhibition is one still resorted to by the man of science, and witnessed by the ordinary observer, with a rational feeling of interest and admiration.

The rapid advances of modern science, and much of the successful issue which has obtained in the researches of its various cultivators, I presume may be fairly attributed, not merely to the increase of philosophical knowledge, but to the greater facility as well as certainty arising to the experimentalist from the improved state of his instruments, combining, as many of them now do, economy with operative convenience and precision. While I have long been a gratified observer of what has been effected by others in these paths of invention, my own pursuits have frequently rendered it expedient to construct a variety of apparatus adapted to demonstration and public lectures, much of which, owing to my local position, there was no hope of obtaining, except through the united agency of mental and operative exertion. The subject of this paper is the offspring of one of these emergencies, and, as during the last twelve years, the instrument has been frequently exhibited in public and adopted in private

use, and always with uniform security and success, I think I am now entitled to speak of its employment with as much confidence as a long experience can warrant.

Whoso recollects the costly aggregation of apparatus, with the cumbrous gas holders to boot, formerly employed in demonstrating the composition of water, even if he has never had occasion to resort to their use, will, I am persuaded, be in a situation to estimate the advantages afforded by the simple and effective instrument I am now to describe; and which, as the *mechanical* agent of forming water, I with deference submit, may (as most of our scientific terms have a Greek origin) be properly denominated the HYDROPLASSON.

In fig. 1, Plate XI., *a* represents a pear shaped glass vessel, the largest diameter of which in the instrument now before me is about eight inches, and the height about ten and a half inches. These dimensions, I believe, will be found adequate for any instrument of the kind, and they may be somewhat reduced at pleasure without any disadvantage. This vessel is manufactured of a sufficient thickness to bear as great a degree of exhaustion as possible, and at the same time to cause it to stand firmly, to promote which objects its weight and form are well suited, especially if the bottom receive a slight depression from the management of the glass blower, or, what is still better, be slightly flattened by grinding. To the mouth or orifice (two and three quarter inches diameter) of the vessel *a*, is securely cemented the brass cap *b* which descends over the neck of the glass to the depth of an inch and a half; the upper circumference of this cap having a deep screw thread cut on its outside, terminating in a shoulder of one tenth of an inch projection, and carrying a leather collar which renders the vessel *a* air-tight when the plate *c* with its milled edge and correspondent screw is properly attached. Through any convenient part of the plate *c*, near its circumference and opposite to each other, pass the two brass wires *d* and *e*, which are insulated from contact with the plate by small corresponding tubes of glass, even with the surface of the plate above and descending to the depth of an inch beneath it. These tubes are plugged with good elastic corks through the centre of which the wires slide freely without admitting air to the vessel *a*. The wires *d* and *e* are bent at right angles towards their inferior extremities, and terminate in small well polished brass balls, which, by turning in the glass tubes before mentioned, may be made occasionally to approximate within three-tenths of an inch of each other. The upper extremity of the wire *d* terminates in another brass ball half an inch in diameter, while the upper end of the wire *e* is again

bent at right angles and terminates in another brass ball of about double the diameter of that on the top of the wire *d.* Through the centre of the plate *c* is made to screw firmly and air-tight, by means of a projecting shoulder and leather collar, the oxy-hydrogen blowpipe or gas deflagrator *f*, the safety tube of which, three-fourths of an inch in diameter and four inches in length, terminates in a jet pipe which may be peirced for the passage of gas with an orifice of from one-tenth to one-twentieth of an inch somewhat larger or smaller at pleasure. The extremity of the jet pipe should descend to within three-fourths of an inch of the knobs *d e*, in such manner that the gas may be delivered in a direct line immediately between the two. The upper end of the safety tube is furnished with a stout brass stop-cock, as seen in the figure, and to the upper extremity of this cock is attached, when required, the large bladder or caouchouc gas holder *g*, fitted with a proper socket and connecting piece, after the usual manner in which unions of this description are effected in many chemical manipulations. The cavity of the safety tube *f* is completely charged with finely granulated silex, obtained by washing from what is commonly denominated *road* or *drift sand*. This granulated silex allows of the free passage of gaseous matter when the instrument is in action, and at the same time effectually prevents the recession of the igneous jet, thus furnishing, as long experience has proved, a medium of absolute security. I may here perhaps be incidentally allowed the mention of an oxy-hydrogen gas deflagrator constructed by me for burning the mixed gases from a single reservoir and in jets of extraordinary dimensions, from the main principle of which apparatus, the safety tube here described is transferred. The principle of the blowpipe alluded to I have had several years in active operation, and by its means have been enabled not only to employ jets with orifices much larger than any other I have ever seen or known used, but to obtain some of the most decisive and brilliant results arising from this most powerful mode of applying heat to, and effecting the decomposition of, refractory bodies.*

* I shall at any time feel a satisfaction in communicating the principle, and describing the structure of this blowpipe, either through the medium of these valuable "Annals," or to private individuals who may be interested in scientific pursuits.*

* We cannot but express our sincere obligations to Mr. Weeks for the liberality thus evinced to his fellow labourers in the field of science; and we are well aware that our readers would be much interested by a description of the blowpipe to which Mr Weeks has alluded; and he may rest perfectly assured that we shall take a particular pleasure in giving it an early publicity, with all the illustrative figures which its ingenious author may think necessary. Edit.

To prepare the hydroplasson for use, detach the gas holder *g*, and having opened the air way of the stopcock *h*, screw the end thereof firmly into the usual recipient for such purposes on the plate of an air pump, when the vessel *a* will be securely supported in an inverted position. Having exhausted the air as far as possible from the vessel *a*, close the stop-cock *h*, remove the instrument from the pump plate, and connect therewith the gas holder *g* charged with the two requisite gases in a state of previous admixture, and in their known proportionate volumes for forming water. This done, adjust the knobs *d e* to a fitting position by turning their respective wires, and then place the instrument within a convenient distance of the electric machine, so that the knob of the wire *e* may be within striking distance of the ball conductor *k*. Now open the air way of the stop-cock *h* partially, so as first to admit a very fine jet of the mixed gases to the vessel *a*. At the same instant place a finger of the left hand on the knob of the wire *d*, while the right hand is employed in turning the electric machine, and a rapid succession of sparks transmitted from the knob *e* to the opposite one *d* will instantly ignite the jet of gas passing directly between them. The admission of gas may now be regulated at pleasure by adjustment of the stop-cock *h*, and in a few seconds a copious dew will be seen to deposit on the inner surface of the vessel *a*, and by continuing the operation water in considerable quantity will be collected at the bottom. It is hardly necessary to observe that when the atmospheric pressure has forced the whole volume of gas from the holder *g*, a second or third may be attached with very little loss of time, for if the apparatus be made air-tight and the operation skilfully managed, a sufficient degree of vacuum will be maintained within the vessel *a* which will not need to be a second time exhausted.

## XLIII. *On the Propagation of Electrical Currents through Liquids.* *By* M. Charles Matteucci.*

*(Continued from page 326.)*

### CHAPTER III.

*On the influence of the extent of surface of electrodes on the intensity of the current they propagate in a liquid.*

It is a well known fact that if an electric current be transmitted through a liquid by two metallic plates, the intensity of the current is increased by the augmentation of the surface

* Translated by Mr. J. H. Lang.

of these plates. M. De la Rive was the first who tried to determine the laws of these phenomena, and he has shown, in a very important memoir, that this augmentation of intensity, which results from the greater extent of the electrodes, increases in a larger proportion than the surface itself, when the current is feeble, and vice versâ when the current is intense. In the undertaking already mentioned, I had myself confirmed the conclusions of the learned philosopher of Geneva.

There yet remains to determine more perfectly the laws of this phenomenon, with regard to the different circumstances depending on the force of the pile, and the conductibility of the reophorous. These researches will be stated in this chapter, combining with them those which establish the reciprocal influence exercised by the three reophorous elements among themselves, viz., the nature and volume of the liquid, and the surface of the electrodes. I shall still follow the same course, viz., beginning with the description of the results obtained from a feeble pile. These experiments were made with platina electrodes, one centimetre in width.

The electrodes were first immersed in the liquid, one centimetre square, they were afterwards successively increased from this same quantity. The distance between the electrodes was eight centimetres. The liquid of the pile and that of the reophorous was distilled water. The pile was such as we have already described.

1*st Table.**

| Number of pairs. | Extent of surface of the electrodes. | | | | |
|---|---|---|---|---|---|
| | 1 c. c. | 2 c. c. | 4 c. c. | 5 c. c. | 6 c. c. |
| 10 | 30 int. | id. int. | 80 int. | 80 int. | 80 int. |
| 20 | 40 | 52 | 80 | ,, | ,, |
| 40 | 45 | 55 | 80 | ,, | ,, |
| 60 | 52 | 80 | 80 | ,, | ,, |
| 100 | 55 | 80 | 80 | ,, | ,, |

I made the liquid reophorous a better conductor by adding a few drops of sulphuric acid, and the results were as follows.

* Very sensitive galvanometer; the intensities of which were determined as far as 40°.

*2d Table.*

| Number of pairs. | Extent of surface of the electrodes. | | | | |
|---|---|---|---|---|---|
| | 1 c. c. | 2 c. c. | 4 c c. | 5 c. c. | 5 c. c. |
| 20 | 42 int. | (45°)? int. | (45°)? int. | (45°)? int. | (45°)? int. |
| 40 | 42 | " | " | " | " |
| 60 | 42 | " | " | " | " |
| 100 | (42°)? | " | " | " | " |

Instead of charging the pile with distilled water, I employed a mixture of 800 parts, by measure, of distilled water, and three of nitric acid. The liquid reophorous was still distilled water as in the first table.

*3d Table.*

| Number of pairs. | Extent of surface of the electrodes. | | | | | |
|---|---|---|---|---|---|---|
| | 1 c. c. | 2 c. c. | 3 c. c. | 4 c. c. | 5 c. c. | 6 c. c. |
| 5 | 21 int. | 33 int. | 47 int. | 59 int. | 75 int. | (45°)? int. |
| 10 | 54 | (44°)? | (52°)? | (62°)? | (68°)? | (72 )? |

In the following tables I employed a better conducting liquid reophorous; it was slightly acidulated water.

In this case I had recourse to the less sensitive galvanometer.

*4th Table.*

| Number of pairs. | Extent of surface of the electrodes. | | | | | |
|---|---|---|---|---|---|---|
| | 1 c. c. | 2 c. c. | 3 c. c. | 4 c. c. | 5 c. c. | 6 c. c. |
| 20 | 6° int. | 8° 10 int. | 10° 13 int. | 12° 18 int. | 14° 24 int. | 18° 26 int. |
| 50 | 12 18 | 16 27 | 19 | 23 | 25 | 26 |
| 60 | 14 24 | | | | | |
| 80 | 16 37 | 23 | 26 | 31 | 32 | 34 |
| 100 | 19ex. | 25 | 28 | 32 | 34 | 36 |

We may easily draw the following conclusions:

1. The intensity of an electric current propagated over a liquid generally increases in proportion to the extent of surface of the electrodes.

2. If the pile have a feeble power of production and propagation, we attain, by this augmentation, a limit beyond which the surface of the electrodes, although augmented, produces no increase of intensity in the current.

3. This limit is sooner attained with feeble piles when they are composed of a great number of pairs, and the liquid reophorous is a good conductor. Hence we gain more by increasing the surface of the electrodes, if the pile be feeble, with but few pairs, and a bad conducting liquid reophorous.

4. When the pile has a great productive force, this limit is further removed, in proportion to the force of the pile, the number of pairs which compose it, and the conductibility of the liquid reophorous.

We will now pass on to the experiments I tried, to establish the laws which regulate the reciprocal influence of three elements of the reophorous, nature and volume of the liquid, and surface of the electrodes. In the experiments I am going to relate, there will be found fresh confirmations of the results already deduced. The pile I employed in these experiments was that already described, its liquid was acidulated water, and its electrodes platina. In the two series that I am about to mention, the electrodes are narrower in one than in the other, that is to say, in the first case they are platina wires, one millimetre wide, and exposing about a centimetre; in the other platina plates, also exposing about a centimetre, but four millimetres wide. The pile had ten pairs, and the liquid reophorous was distilled water. This liquid was contained in a rectangular box of varnished wood, one centimetre wide, and two deep. The following are the results obtained by comparing the action of the length of the liquid bed, with the surface of the electrodes on the intensity of the current.

1st *Table.**

| Length of the liquid bed. | Narrow electrodes. | Wide electrodes. |
|---|---|---|
| $2^m$ | 1 Int. | 1 Int. |
| 1 | 1 „ | 1 „ |
| 0 ,50 | 2 „ | 2 „ |
| 0 ,15 | 6 „ | 10 „ |
| 0 ,05 | 10 „ | 32 „ |
| 0 ,025 | 21 „ | (46°) ? „ |
| 0 ,012 | 36 „ | (72 ) ? „ |
| 0 ,006 | 75 „ | (90 ) ? exc. |

I increased the pile to twenty pairs and obtained:

| Length of the liquid bed. | Narrow electrodes. | Wide electrodes. |
|---|---|---|
| $2^m$ | 1 Int. | 1 Int. |
| 1 | 2 „ | 2 „ |
| 0 ,50 | 3 „ | 3 „ |
| 0 ,10 | 23 „ | 28 „ |
| 0 ,05 | 52 „ | (54°) ? „ |
| 0 ,025 | (54°)? „ | (74 ) ? „ |

* Sensitive galvanometer.

With the first pile of ten pairs and a better conducting liquid in the trough, the solution of a centième of nitre in the distilled water, I obtained the following results;

| Length of the bed. | Narrow electrodes |
|---|---|
| 2$^m$ | 18 Int. |
| 1 | 45 |
| 0 ,50 | (56°)? |
| 0 ,20 | (90 )? |

With three pairs, in the last liquid, I obtained:

| Length of the bed. | Narrow electrodes. |
|---|---|
| 2$^m$ | 10 Int. |
| 1 | 16 |
| 0 ,20 | 32 |
| 0 ,50 | 55 |

I shall mention some more experiments which establish this reciprocal influence of the extent of the electrodes and the length of the liquid bed. I have in these experiments varied also the number of pairs. The pile was that already described for a constant force; the liquid reophorous distilled water; the length of the bed or distance between the two electrodes was four centimetres.

1*st. Table.*

| Extent of surface of the electrodes. | Number of pairs. | | |
|---|---|---|---|
| | 2 | 4 | 8 |
| ½ cen. square. | 2 int. | 4 int. | 7 int. |
| 1 | 3 | 5 exc | 9 |
| 2 | 5 | 9 | 18 |
| 3 | 6 | 10 | 21 |
| 5 | 8° | 16 50 | 33 |

I increased the distance between the electrodes to sixteen centimetres and altered nothing in the rest of the experiments.

2*d. Table.*

| Extent of surface of the electrodes. | Number of pairs. | | |
|---|---|---|---|
| | 2 | 4 | 8 |
| ½ cen. square. | 2 int. | 5 int. | 9 int. |
| 1 | 3 | 6 | 15 |
| 2 | 4 | 8 | 16 |
| 3 | 5 | 10 | 17 |
| 5 | 6 scarcely. | 10 exc. | 19 |

I also made other experiments with a greater number of pairs, comparing the extent of the electrodes with a liquid bed whose length in one case was double that in the other. The liquid reophorus was distilled water

*3d. Table.*

| Extent of electrodes. | Pile of 10 pairs. | | Pile of 30 pairs. | |
|---|---|---|---|---|
| | Simple bed. | Double bed. | Simple bed. | Double bed. |
| 1 cen. square. | 18 int. | 12 int. ex. | 49 int. | 36 int. |
| 2 | 23 | 16 | 73 | 45 |
| 3 | 30 | 18 | (43°)? | 49 |
| 4 | 34 ex. | 21 | (45 )? | 55 |

Finally I compared the length of the liquid bed with the extent of the electrodes, using two different piles, in one of which the surface of the voltaic pairs was double that of the other. The liquid reophorus was distilled water and that of the pile pump water. The surfaces of the voltaic pairs are those which we have already described: they are 128 centimetres square.

*4th. Table.*

Simple surface of the pairs of the pile.

| Length of the liquid bed. | Narrow electrodes. | Wide electrodes. |
|---|---|---|
| $1^m$,40 | 3 int. | 3 int. ex. |
| 1 ,0 | 3 ex. | 4 |
| 0 ,8 | 3 | 5 |
| 0 ,4 | 6 | 10 |
| 0 ,2 | 11 | 8 |
| 0 ,1 | 16 | 27 |
| 0 ,05 | 20 | 42 |

Double surface of the pairs of the pile.

| | | |
|---|---|---|
| $1^m$,10 | 6 int. | 7 int. |
| 1 ,0 | 8 | 10 |
| 0 ,8 | 10 | 12 |
| 0 ,4 | 14 | 24 |
| 0 ,2 | 22 | 40 |
| 0 ,1 | 31 | (44°)? |
| 0 ,05 | 45 | " |

I consider it useless to add any more experiments: from these we may draw the following conclusions—

1. With a certain given pile, we find constantly that the influence exercised on the intensity of the current, by one or

two of the reophorous elements, is such that the augmentation of the intensity afterwards produced by the other, or two other elements, increases in proportion to the first increase.

2. These variations increase with the force, extent, and number of pairs of the pile.

Hence if we make the currents more intense by a greater extent of the electrodes, we perceive the effect of the diminution of the liquid bed increase proportionally. If the liquid be a better conductor, the influence of the extent of the electrodes and of the diminution of the thickness of the liquid bed is always greater. In the chapter of metallic diaphragms, we shall have occasion to return to the influence of the nature of the metals employed as electrodes on the intensity of the current they transmit.

## CHAPTER IV.

*On the influence of heat over the conducting power of liquids.*

Among the phenomena studied by M. Marianini, there is that of the action of heat on the liquids in which the voltaic pairs are immersed. He intended thus to study the influence of heat on the conducting power of liquids. We have elsewhere proved the defects of his procedure. The conclusions that he had come to, were: that the conducting power increases in proportion as the temperature increases, and that this augmentation is greater for bad than good conducting liquids. Since the work of M. Marianini, nothing has been added, except what I have published myself in the *Bibliotheque Universelle* de Genève. In the new researches, that I am about to relate, it will be seen I have been able to confirm and extend them. The pile I employed was for constant force, I used a rectangular porcelain box for heating the liquid in. I do not think it necessary to mention the results obtained in studying the effect of the variable force of the pile on the conductibility of the liquid at different temperatures.

The degree of the heat of the liquid may be compared, in that respect, to the other conditions of the reophorous, nature of the liquid, extent of the electrodes, and volume of the liquid, which we have already studied. I confine myself to describing some experiments made to discover the action of heat on the conducting power of different liquids: my first experiments were with distilled water, and are as follows:

| Degrees of Raumer's Thermometer. | Conducting power. | |
|---|---|---|
| 0° | 6° | ,, int. |
| + 5 | 8 | ,, |
| +10 | 10 | ,, |
| +15 | 12½ | 13 |
| +20 | 15 | 16,5 |
| +25 | 17 | 19,5 |
| +30 | 18 | 21 |
| +35 | 21 | 25 |
| +42 | 25 | 31,5 |
| +55 | 30 | 40 |
| +65 | 33 | 47 |
| +80 | 36 | 55 |

Instead of distilled water, I took a solution in the same liquid of a hundredth part of sal ammoniac and a solution of $\frac{1}{100}$ of iodine of potassium. In this case I made use of the less sensitive galvanometer.

| Degrees of the Thermometer. | Conducting power of a solution. $\frac{1}{100}$ of sal ammoniac. | $\frac{1}{100}$ of iodure of potassium. |
|---|---|---|
| 0° | 38° | ,, |
| + 5 | ,, | 17° |
| + 6 | 42 | ,, |
| +15 | 47 | 20 |
| +22 | 50 | ,, |
| +30 | ,, | 27 |
| +38 | 55 | ,, |
| +40 | ,, | 31 ex. |
| +50 | 56 | 32 |
| +55 | 58 | ,, |
| +60 | 62 | 42 |
| +70 | ,, | 45 ex. |
| +75 | 63 | 47 |
| +80 | 63 | 50 |

I also discovered that a solution saturated with sal ammoniac at 0° has the same conducting power, from + 20° to + 80°. The conclusions that may be drawn from these experiments, and others which I think it useless to describe, are the following:—

1. The conductibility increases in general by the augmentation of the temperature;
2. The effect of heat on the conducting power of liquids attains a certain limit;
3. This limit takes place at as much more elevated a temperature as the conductibility of the liquid is less.

4. Hence, the increase of conductibility is, generally, as much more rapid for the first augmentations of the heat as this conductibility is better.

In studying the conductibility of a liquid which has been warmed, during its cooling I had an opportunity of observing an extremely curious phenomenon: it was that of the persistence of a conductibility superior to that of the degree of temperature at which the liquid was and which it had not acquired in coming to it. It was particularly with a solution of nitro-sulphuric acid that I verified this fact.

| Degrees of the Thermometer. | Conductibility of the solution that is heated. | Conductibility of the same solution when cooled. |
|---|---|---|
| 0° | 22° | 24° |
| +15 | 31 | 35 |
| +22 | 36 | 38 ex. |
| +38 | 40 | 44 |
| +50 | 44 | 45 |

After leaving this solution at 0° for a certain time, its conductibility returned to 22°, and by then heating it, the conductibilities were reproduced as in the first series. Cooling it again, the persistence appeared afresh and was even increased. In the second case the solution must be maintained for a longer time at 0° in order to retake the conductibility of 22°.

This phenomenon deserves a much deeper study; for it establishes connexions between the action of heat, the molicular arrangement, and the conducting power for the electric fluid.

We cannot help seeing the analogy that it has with the property possessed by several solutions, of keeping a greater quantity of salt dissolved than they would when heated; it is even the case with water, phosphorus, &c. &c. which under certain circumstances do not become solid, though at a temperature below that of their solidification.

## CHAPTER V.

*On the modifications experienced by the electric current in its passage over liquid conductors, interrupted by metallic diaphragms or by beds of a different liquid.*

We are indebted to M. de la Rive for the first researches on this point; the following are the results he obtained:—

1. The loss produced by metallic diaphragms is almost nothing, when the current is very energetic and proceeding from a very great number of pairs;

2. The intensity of the current diminishes, by traversing the same number of diaphragms, in a proportion as much greater as its original intensity is less;

3. Of two currents having the same intensity, one originally and the other after having traversed one or more metallic plates, the first diminishes much more, by the interposition of a plate, than the second which has already passed over similar plates.

I have attempted to complete the study of this phenomenon, by determining the influence of the different elements constituting the force of the pile and conductibility of the reophorous, over this loss produced by the diaphragms. In the first researches I am about to describe, I always used the pile already so often mentioned, and platina diaphragms. All these diaphragms were made of the same plate and fixed by a varnish to the partitions of my troughs. I made the current of the pile traverse the liquids which were already interrupted by the diaphragms. I took away the diaphragms one by one, coming at last to the liquid alone. I always took care to be certain that the force of the pile remained almost constant, for the short time employed in these experiments. In the first table, the pile was charged with distilled water, the trough was two decimetres long, one centimetre wide, and twenty-five millimetres deep. Three platina diaphragms divided the trough full of distilled water.

1*st. Table.*

| Number of pairs. | Plates. | | | |
|---|---|---|---|---|
| | 0 | 1 | 2 | 3 |
| | Int. | Int. | Int. | Int. |
| 80 | (41°)? | (41°)? | 55 | 45 |
| 40 | 31.50 | 31 | 25 | 22 |
| 20 | 14 | 12.50 | 11 | 9 |
| 10 | 7 | 6 | 5 | 4 |

Placing a better conducting liquid, pump water, in the trough, and keeping all the other arrangements the same, I obtained the following results:—

2*d. Table.*

| Number of pairs. | Plates. | |
|---|---|---|
| | 0 | 1 |
| | Int. | Int. |
| 20 | (50°)? | 75 |
| 10 | 47 | 48 |
| 5 | 14 | 5 |

Making in another trough, the height of the liquid 0.04, I obtained the following results:—

*3d. Table.*

| Number of pairs. | Plates. | |
|---|---|---|
| | 0 | 1 |
| | Int. | Int. |
| 20 | (54°)? | (44°)? |
| 10 | (42 )? | 33 |
| 5 | 21 | 7 exc. |

By halving the length of the trough filled with distilled water, as in the first table, we have:—

*4th. Table.*

| Number of pairs. | Plates. | |
|---|---|---|
| | 0 | 1 |
| | Int. | Int. |
| 80 | (50°)? | (44°)? |
| 60 | (48 )? | (42 )? |
| 40 | (44 )? | 52 |
| 20 | 45 | 25 |
| 10 | 19,50 | 10 |

I have also tried to vary the intensity of the current, by employing smaller or larger electrodes.

I shall give two tables in which the experiment was arranged as in the first: the only difference being in the surface of the electrodes, which were four millimetres wide and two centimetres deep. The following are the results:

*5th. Table.*

| Number of pairs. | Plates. | |
|---|---|---|
| | 0 | 1 |
| | Int. | Int. |
| 10 | 10 | 9 |
| 20 | 29 | 25 |
| 30 | 45 | 44 |
| 40 | (42°)? | (41°)? exc. |

With electrodes one centimetre wide and two centimetres high, I obtained:

*6th. Table.*

| Number of pairs. | Plates. | |
|---|---|---|
| | 0 | 1 |
| | Int. | Int. |
| 10 | 16.50 | 14 |
| 20 | 40 | 34,50 |
| 30 | 75 | 68 |
| 40 | (43°)? | (41°)? |

It would be useless to give any more experiments: those I have already described are sufficient for us to deduce the following consequences :—

1. The loss experienced by a current traversing a metallic diaphragm placed in a trough of liquid, is as much less, as the intensity of this current is increased by a greater number of pairs.

2. The loss increases in general with the number of diaphragms, but it does not increase proportionally to this number, and the relative loss is as much less as the pile consists of a greater number of pairs:

3. When the intensity of the current is increased, either by a greater volume of liquid current, a shorter liquid bed, its better conductibility, or a greater extent of electrodes, the loss produced by the diaphragms is much greater than that suffered by a current whose initial intensity* may be equal, but not owing to a greater number of pairs, rather than to better conditions of the reophorous.

I shall now give the results obtained by making the liquid of the pile a more active and better conductor; it consisted of a solution of nitro-sulphuric acid in distilled water. The trough was filled with distilled water.

*7th. Table.*

| Number of pairs. | Plates. | |
|---|---|---|
| | 0 | 1 |
| 20 | 21 int. | 15,50 int. |
| 30 | 33 | 23 |
| 40 | 47 | 33 |
| 50 | 75 | 45 |
| 60 | (48°)? | 64 |

* By the initial intensity of the current, I mean that which it would have without traversing the metallic diaphragma.

The following were the results of another experiment:

*8th. Table.*

| Number of pairs. | Plates. | | |
|---|---|---|---|
| | 0 | 1 | 2 |
| 20 | 15 int. | 11 int. | 9 int. |
| 30 | 23 | 18 | 14 |
| 40 | 31 | 27 | 18 |
| 50 | 40 | 34 | 23 |
| 60 | 55 | 45 | 31 |

Hence we may conclude.

1. That the loss produced by the diaphragms, does not cease increasing when the intensity of the initial current increases, and we find, by comparing equal currents in this last table and in the first, which differ in the number of pairs and nature of the liquid of the pile, that these losses are much greater for a current proceeding from a less number of pairs.

2. This loss, with piles made of a few pairs, is far from diminishing in proportion to each diaphragm.

This result is much more evident when the liquid of the pile is more active, and the number of pairs less. The following is an example with the same pairs, but a more active liquid.

*9th. Table.*

| Number of pairs. | Plates. | |
|---|---|---|
| | 0 | 1 |
| 2 | 7 int. | 4 int. |
| 4 | 63 | 33 |
| 8 | (86°)? | (72°)? |

Causing the current, of a single pair of a Wollaston pile, whose zinc had a surface of 720 c.c., and charged with a very acid liquid, to pass through the same trough, I obtained 13° with no plate and 4° with 1 plate. Hence, it is very evident that the resistance produced by the diaphragms diminishes in proportion to the number of pairs, by which the current is caused. These results may be expressed in a general manner. "The number of pairs forms the bounds at which the loss produced by the diaphragms ceases, varies proportionally to the conductibility of the liquid placed between the pairs of the pile and those of the reophorus, that is to say, with the volumes, conductibility of the liquid, and extent of the electrodes."

I also endeavoured to find out what influence was produced by the different nature of the metal of the diaphragms; for this purpose I employed my pile, but charged with a nitro-sulphuric solution: in the trough I had pump water. The following is the result.

*10th. Table.*

| Number of pairs. | Plates. 0 | Plates. 1 | |
|---|---|---|---|
| 20<br>10<br>5 | (100°)? int.<br>( 65°)?<br>45 | (91°)? int.<br>(56°)?<br>29 | Platina. |
| 20<br>10<br>5 | id. | (94°)?<br>(62°)?<br>32 ex. | Silver. |
| 20<br>10<br>5 | id. | (94°)?<br>(60°)?<br>30 | Brass. |
| 20<br>10<br>5 | id. | (98°)?<br>(63°)?<br>36 | Copper. |
| 20<br>10<br>5 | id. | (84°)?<br>(60°)?<br>36 | Lead. |
| 20<br>10<br>5 | id. | (98°)?<br>(63°)?<br>38 | Zinc. |

It is very clearly seen, by this table, that the the nature of the metallic diaphragms, has a great influence over the loss experienced by the current in traversing them. This influence which is also exercised, and in the same manner when the metals are employed as electrodes, is such that the loss produced in the current is as much less as the metal is more easily attacked by the liquid conductor: it is even greater for piles of few pairs; it is also greater if the liquid reophorous be a better conductor. M. De la Rive was the first who remarked this property of the different metals.

Having shown the results obtained from studying the transmissibility of the electric current through a liquid trough, divided into several compartments by metallic diaphragms, I shall now describe the modifications experienced by this current, when it is obliged to traverse the same liquid trough, divided in the middle by a liquid bed, having a greater

conductibility than that of the liquid trough in which the electrodes are immersed. It will be useless to describe the apparatus, having always my varnished wooden boxes. These boxes are divided at ten centimetres, by a piece of animal membrane; the length of the trough was one meter; the pile was that for constant force of eight pairs.

1*st. Table.**

| Length of the less conducting liquid bed. | Degrees. | Intensity.† | In the good conducting liquid bed, 0m,2 long. |
|---|---|---|---|
| 0m,2 | 50 | | |
| 0 ,4 | 35 | 52 | (66°)? |
| 0 ,6 | 25 ex. | 32 | |
| 0 ,8 | 20 | 23 | |

In the fifth and sixth cavities I place the good conducting liquid; and the inferior conducting liquid occupied the other parts of the trough. Hence, the bed of the good conductor is in the middle of the trough. The following are the results obtained by making the current traverse the trough composed of the intermediate bed, and of a different number of lateral ones. It is useless to say that the electrodes were immersed in the lateral cavities, in which was the bad conductor.

2*d. Table.*

| Bad conductor. | | Good conductor. | Degrees. | Intensity. |
|---|---|---|---|---|
| 0m,2 | + | 0m,2 | 33° ex. | 48 |
| 0 ,4 | + | id. | 27 | 34.5 |
| 0 ,6 | + | id. | 21 | 23 |
| 0 ,8 | + | id. | 17 | 20 |

I placed in the intermediate cavities a still better conducting liquid; the current in this was 82° ex?, and I obtained the following results.

| Bad conductor. | | Good conductor. | Degrees. | Intensity. |
|---|---|---|---|---|
| 0m,2 | + | 0m,2 | 38° | 64 |
| 0 ,4 | + | id. | 30 | 40 |
| 0 ,6 | + | id. | 24 | 30 |
| 0 ,8 | + | id. | 19 ex. | 23 |

Two important results may be deduced from these former tables.

* More sensitive galvanometer.

† The word intensity, used in the table, signifies the force corresponding to the arc expressed in degrees.

1. If we compare the losses sustained by different currents, produced by the same pile of constant force, but having a different intensity from the variable length of the less conducting liquid bed which it traverses, we find that these losses, produced by the intermediate good conducting bed, are as much less as the primitive currents are reduced to a less intensity, by a greater length of the bad conducting course.

2. These losses follow the same law when the intermediate liquid bed becomes a better conductor. We find, however, that these losses diminish in this case still more rapidly. The first law is also true for metallic diaphragms; the second establishes the influence of the conductibility of the diaphragm, and partly explains the difference we find between the various metals employed as diaphragms.

I shall now give the results obtained from studying these modifications, after having changed the intensity of the initial current by a greater extent of electrodes, and a better conductibility of the liquid of the extreme beds.

I shall commence with two tables, which differ in regard to the former ones we have given, only in the pile being reduced to four pairs.

*3d. Table.*

| Bad conducting liquid. | Degrees. | Intensity. | Length of the good conducting liquid. | Degrees. |
|---|---|---|---|---|
| 0m,2 | 21 | 23 | 0m,2 | (42° ex.)? |
| 0 ,4 | 14 | 13 | | |
| 0 ,6 | 10 ex. | 10 | | |
| 0 ,8 | 7 | 7 | | |

| Bad conducting bed. | | Good conducting bed. | Degrees. | Intensity. |
|---|---|---|---|---|
| 0m,2 | + | 0m,2 | 14 | 15 |
| 0 ,4 | + | id. | 10 | 10 |
| 0 ,6 | + | id. | 8 | 8 |
| 0 ,8 | + | id. | 6 | 6 |

Making the intermediate liquid a better conductor, the result modified in the following manner: in this intermediate bed the current was at 52°

*4th. Table.*

| Inferior conducting liquid. | | Good conducting liquid. | Degrees. | Intensity. |
|---|---|---|---|---|
| 0m,2 | + | 0m,2 | 16 | 18 |
| 0 ,4 | + | id. | 12 | 12.50 |
| 0 ,6 | + | id. | 9 | 9 |
| 0 ,8 | + | id. | 7 | 7 |

If we now compare these two last tables with the former ones that we have given, in which the pile had eight pairs, we shall find the losses to be proportionally greater as the pairs of the pile are less numerous: in that, they follow the same general laws we have already deduced. We now see that when the initial intensity of the current is increased by the better conductibility of the lateral liquid, or by a greater extent of the electrodes, the losses become proportionally greater. In the following tables we shall also be able to compare the effect of the number of pairs.

Pile of fifty pairs, charged with water slightly salted.

| Inferior conducting liquid. | Degrees. | Good conducting liquid. | Degree. |
|---|---|---|---|
| $0^m,2$ | (58) ? | $0^m,2$ | (66) ? |
| 0 ,4 | (54) ? | | |
| 0 ,6 | (50) ? | | |

| Inferior conducting liquid. | | Good conducting liquid. | Degrees. |
|---|---|---|---|
| $0^m,2$ | + | $0^m,2$ | (55) ? |
| 0 ,4 | + | id. | (50) ? |

With the same pile and an inferior conducting liquid in the lateral cavities I obtained the following results.

| Inferior conducting liquid. | Degrees. | Intensity. |
|---|---|---|
| $0^m,2$ | 44 | ? |
| 0 ,4 | 38 | 64 |
| 0 ,6 | 34 | 49 |
| 0 ,8 | 32 | 45 |

| Inferior conducting liquid. | | Good conductor. | Degrees. | Intensity. |
|---|---|---|---|---|
| $0^m,2$ | + | $0^m,2$ | 44 | ? |
| 0 ,4 | + | id. | 38 | 64 |
| 0 ,6 | + | id. | 33 ex. | 47 ex. |
| 0 ,8 | + | id. | 30 ex. | 40 ex. |

Pile for constant force of eight pairs and platina electrodes wider than the preceding.

| Inferior conducting liquid. | Degrees. | Intensity. |
|---|---|---|
| $0^m,2$ | 40 ex. | 75 ex. |
| 0 ,4 | 28 | 36 |
| 0 ,6 | 19 | 22 |
| 0 ,8 | 15 | 16,50 |

In the intermediate good conducting liquid, the current was (66°)?

| Inferior conducting liquid. | | Good conductor. | Degrees. | Intensity. |
|---|---|---|---|---|
| 0$^m$,2 | + | 0$^m$,2 | 33 | 47 |
| 0 ,4 | + | id. | 22 | 27 |
| 0 ,6 | + | id. | 16 | 18 |
| 0 ,8 | + | id. | 14 | 15 |

Pile of four pairs with electrodes four times the extent of the preceding ones.

| Inferior conducting liquid. | Degrees. | Intensity. |
|---|---|---|
| 0$^m$,2 | 29 ex. | 38 |
| 0 ,4 | 18 | 21 |

| Inferior conducting liquid. | | Good conducting liquid. | Degrees. | Intensity. |
|---|---|---|---|---|
| 0$^m$,2 | + | 0$^m$,2 | 24 | 30 |
| 0 ,4 | + | id. | 15 ex. | 17 |

This same pile of four pairs was employed under the same conditions with leaden electrodes the same extent as the preceding. I obtained the following results.

| Inferior conductor. | Degrees. | Intensity, |
|---|---|---|
| 0$^m$,2 | 34 ex. | 50 |
| 0 ,4 | 20 ex. | 24 |
| 0 ,6 | 16 ex. | 18 |
| 0 ,8 | 14 | 15 |

| Inferior conducting liquid. | | Good conductor. | Degrees. | Intensity. |
|---|---|---|---|---|
| 0$^m$,2 | + | 0$^m$,2 | 26 | 33 |
| 0 ,4 | + | id. | 18 | 22 |
| 0 ,6 | + | id. | 14 | 15 |

With the same pile of four pairs, narrow electrodes, and a better conducting liquid in the lateral cavities, I obtained

| Inferior conducting liquid. | Degrees. | Intensity. |
|---|---|---|
| 0$^m$,2 | 23 | 29 |
| 0 ,4 | 15 exc. | 16,50 |

| Inferior conducting liquid. | | Good conductor. | Degrees. | Intensity. |
|---|---|---|---|---|
| 0$^m$,2 | + | 0$^m$,2 | 18 | 21 |
| 0 ,4 | + | id. | 13 | 14 |

There is yet one other circumstance to be considered, which influences the force of the pile, that is, the extent of surface of the pairs. The pile that I employed was that of eight pairs already described; its liquid was pump water. The liquids of the trough and electrodes were as in the first table.

| Pairs of single surface. | | | Pairs of double surface. | |
|---|---|---|---|---|
| Bad liquid conductor. | Degrees. | Intensity. | Degrees. | Intensity. |
| $0^m$,2 | 38 | 64 | 54 | „ |
| 0 ,4 | 25 | 32 | 23 | 45 |
| 0 ,6 | 19 exc. | 23 | 24 | 30 |
| 0 ,8 | 15 exc. | 16,50 | 18 | 21 |
| 1 ,0 | 13 | 14 | 15 | 16,50 |

| | Degrees. | Intensity. | Degrees. | Intensity. |
|---|---|---|---|---|
| $0^m$,2 + $0^m$,2 good conductor. | 32 exc. | 45 | 36 | 56 |
| 0 ,4 id. | 21 | 25 | 25 exc. | 32 |
| 0 ,6 id. | 17 | 20 | 19 exc. | 22 exc. |
| 0 ,8 id. | 15 | 16 exc. | 16 | 18 |

We see by this last, that the influence of the extent of the pairs does not differ from that of the nature of the liquid of the pile. Thus, we may conclude that the number of pairs being given, having like conditions in the reophorous, whether we vary the liquid of the pile or the extent of the voltaic pairs, if by these two ways we give the current the same intensity, the loss that it experiences is equal, and generally proportional to this intensity.

I also wished to try if the length of the intermediate good conducting bed had any influence over these losses. I found, in general, that this length is indifferent, provided it be not increased so as to make the middle bed an inferior conductor to the lateral ones.

There remains yet to discover, if the disposition of this good conducting bed in the middle of the trough, have any influence over these losses. It is, indeed, very evident that, by leaving the same electrodes in the same extreme cavities, I could divide the good conducting bed into two or three parts, thus interpolating with the beds of the inferior conductor.

I tried several experiments of this kind, and I have never been able to perceive but a loss of a few degrees greater in the case of the interpolated beds. The inferior conductor is always the liquid in which the electrodes are immersed.

Hence, I may conclude from these researches:

1. That the current experiences a loss, when a good conducting liquid bed is placed in the middle of a liquid trough, having a conductibility less than that of the middle bed.

2. These losses diminish proportionally when the intensity of the current is increased by a greater number of pairs.

If on the contrary, this intensity increase by the less length of the liquid trough, by a greater extent of the elec-

trodes, or by the better conductibility of the liquid of the trough, the losses are proportionally greater;

3. These losses are independent of the length of the intermediate bed, and diminish very rapidly for a second good conducting bed, in the case in which we divide in two the bed of the middle which we have considered in these experiments.

I shall now take a case opposed to that we have just considered, viz., a liquid trough having in its middle an inferior conducting bed. I at first thought the intensity of the current transmitted by such a trough, would have been that due to the inferior conducting bed. The experiments I am going to mention have modified in part this principle; the first fact was observed on changing the extent of the electrodes and their nature. The experiment was arranged thus: one of my boxes, 1 metre long, was divided into three cavities, of which the extreme ones were $0^m,4$ long: pieces of membranes were always placed between these separations; first I filled the three cavities with distilled water; caused the current of my pile for constant force of eight pairs to pass in the middle cavity, at one time placing the electrodes within the membranes, and at another without. I always made this double proof to determine if there were any loss occasioned by the membranes. I obtained 12° in both cases: I emptied the extreme cavities by a small pipe, and filled them with pump water; the electrodes were in the lateral cavities at $0^m,1$ from each membrane; hence, the bed traversed was $0^m,4$ long, of which $0^m,2$ of distilled water in the middle, and $0^m,1$ of pump water on each side. The current was still 12°, changing the electrodes and putting those whose surface was three times that of the preceding ones, the current rose to 14°=15 int. The same electrodes placed in the middle cavity of distilled water, I had 14°=15. I also renewed the electrodes, putting a plate of brass equal to the former ones of platina, the current was 17°=19.5 in the cavity of distilled water, and the same in that composed of distilled and pump water. Thus far we see that, when a current is transmitted over a liquid trough composed half of an inferior conductor placed in the middle, and half of a good conductor placed on each side of the former, its intensity is equal to what would have been obtained if the current had only traversed the mean cavity of the inferior conductor. The difference produced in this case by the plates of brass is an evident proof of the influence of the natural conductibility of the material of the electrodes on their action as diaphragms.

I then tried to vary the lengths of the good conducting lateral beds, always within such limits as not to become, by

their great lengths, inferior conductors to the middle cavity. The general result at which I arrived after a great number of experiments, may be expressed in the following manner; "whatever may be the conducting power of the good conducting liquid, in which the electrodes are immersed and which compose the lateral beds of the trough, having for its middle the inferior conducting liquid, and whatever may be the force and number of pairs of the pile, the current which traverses these compound troughs has an intensity, which although variable, is never superior to that of this same current when it traverses the mean inferior conducting bed alone. This intensity varies, diminishing from the *maximum* we have established; and these losses arise by increasing the length of the lateral beds of the good conductor; they are independent of the force of the pile, varying with the length of these lateral beds, and that in proportion to the variable intensity
acquire by changing the electrodes and the
d conducting liquid of the middle." Giving
e extreme cavities a still greater conducting
g, in every respect, the same pile, the same
ame liquids in the middle cavity, the current
the compound trough, suffers, on comparing
the extreme cavities, losses as much less as
the conductibility is greater. There now only remains for me to mention some of the experiments tried, and from which I have deduced these results. Those showing the influence of the extent of the electrodes, have already been described. The following are others, for the variable length of the lateral beds. I took a pile of forty pairs, which (zinc and copper) were those we have described so many times. The liquid of the pile was water slightly acidulated with nitro-sulphuric acid. The liquids of the trough were distilled water in the middle, and pump water in the extreme cavities.

*1st. Table.*

| Good conductor. | | Bad conductor. | Degrees. | Intensity. |
|---|---|---|---|---|
| $0^m,2$ | + | $0^m,2$ | 10 | id. |
| 0 ,4 | + | id. | 9 | id. |
| 0 ,8 | + | id. | 8 | id. |

Making the liquid of the mean cavity a better conductor, I obtained:

*2d. Table.*

| Good conductor. | | Bad conductor. | Degrees. | Intensity. |
|---|---|---|---|---|
| $0^m,2$ | + | $0^m,2$ | 15 | 16,5 |
| 0 ,4 | + | id. | 12 | 12,5 |
| 0 ,8 | + | id. | 12 | 12,5 |

In another experiment I employed the pile for constant force and obtained the following result:

*3d. Table.*

| Good conductor. | | Bad conductor. | Degrees. | Intensity. |
|---|---|---|---|---|
| $0^m,2$ | + | $0^m,2$ | 26 | 33 |
| 0 ,4 | + | id. | 19 | 22,5 |
| 0 ,8 | + | id. | 12 | 12,5 |

I changed the liquid in the mean and extreme cavities and obtained the following results:

*4th. Table.*

| Good conductor. | | Bad conductor. | Degrees. | Intensity. |
|---|---|---|---|---|
| $0^m,2$ | + | $0^m,2$ | 32 | 43 |
| 0 ,4 | + | id. | 26 | 33 |
| 0 ,6 | + | id. | 23 | 29 |
| 0 ,8 | + | id. | 21 | 25 |

The current maximum is always equal to the current we have in the single cavity of the middle. I shall also mention some other results obtained by using the pile of eight pairs already described, but with very wide surfaces: its liquid was pump water. In the four following tables, the difference is in the nature of the liquids of the middle and sides, which I have changed at the same time and in the same way.

*5th. Table.*

| Good conductor. | | Bad conductor. | Degrees. | Intensity. |
|---|---|---|---|---|
| $0^m,2$ | + | $0^m,2$ | 13 exc. | 14 |
| 0 ,4 | + | id. | 11 | 11 |
| 0 ,8 | + | id. | 10 | 10 |

*6th. Table.*

| | | | | |
|---|---|---|---|---|
| $0_m,2$ | + | $0^m,2$ | 15 | 16,5 |
| 0 ,8 | + | id. | 12 | 12,5 |

*7th. Table.*

| | | | | |
|---|---|---|---|---|
| $0^m,2$ | + | $0^m,2$ | 17 | 19.5 |
| 0 ,4 | + | id. | 16 | 18 |
| 0 ,8 | + | id. | 15 | 16 |

*8th. Table.*

| | | | | |
|---|---|---|---|---|
| $0^m,2$ | + | $0^m,2$ | 17 | 20 |
| 0 ,4 | + | id. | 16 | 18 |
| 0 ,8 | + | id. | 16 | 18 |

Finally, I connected in two tables the experiments which show the difference occasioned by the conductibility of the liquid of the lateral cavities. The pile was the same as in the preceding experiments.

*9th. Table.*

| Good conductor. | | Bad conductor. | Degrees. | Intensity. |
|---|---|---|---|---|
| $0^m,2$ | + | $0^m,2$ | 14 | 15 |
| 0 ,8 | + | id. | 9 | 9 |

In the latter, the current was 32°=45 in $0^m,4$ of the lateral good conductor.

*10th. Table.*

| Good conductor. | | Bad conductor. | Degrees. | Intensity. |
|---|---|---|---|---|
| $0^m,2$ | + | $0^m,2$ | 14 | 15 |
| 0 ,8 | + | id. | 13 | 14 |

In this the current is $0^m,4$ of the good conductor=44°. We see by these experiments that in a trough composed of different liquids, the intensity of the current which traverses it, is in general determined by that which might traverse the inferior conducting portion. This intensity is enfeebled by the length of the good conducting bed that it has to traverse before entering the intermediate cavity of the bad conductor. Hence, it is a loss produced by the change of the conductor, and which appeared when the current was previously enfeebled.

All the results contained in this first part of my work and at which I had arrived some time ago, agree with those M. De la Rive has deduced in a general manner starting from a principle which belongs to them, viz: that of the recomposition of two fluids by the same pile. M. Peltier has supported by some very ingenious experiments, the ideas of the philosopher of Geneva. To increase the number of pairs of a pile, when its current is obliged to traverse a bad conductor, is for these skilful philosophers to render more difficult the passage of a portion of the current in the same pile.

It is thus we must arrive at a limit in this number of pairs, beyond which the portion of the current which circulates in the liquid reophorous ceases to increase, and that as much sooner as the liquid of the pile is a worse conductor, and on the other hand the conditions of conductibility of the reophorous are better.

## CHAPTER VI.

*On the difference of transmissibility according to the direction of the current.*

In a Memoir published, two years ago, in Italian, I had begun the study of certain facts, which demonstrate the difference in the transmissibility of the electric current by a certain reophorous according as the same modifications be found attached to the positive or negative pole. The learned have

been for some time in possession of three facts of this kind: they are due to M. M. Porret, Becquerel, and Marianini. I have discovered others, which appear to me as interesting in themselves and which become more so, since they put us in the way of connecting them together and uniting them under the same principle.

I shall treat of them in several sections.

1st. Section. *Unequal extent of the two electrodes.*

This fact, the first discovery of which is due to M. Marianini, deserves a very deep study. No instrument is more fit for the researches I am about to describe in this chapter, than the double galvanometer. Its action is to compare the relative conductibility of the two reophorous systems, and afterwards to show in which of the two, the current is best transmitted.

We must prepare these experiments with all possible care, that is to say we must endeavour to have all the parts of the two systems perfectly identical: the only difference should be that, we are about to consider. We should never commence an experiment without being certain, that with the exception of this difference alone, the currents are equal in the two reophorouses. The whole apparatus consists in two varnished wooden boxes, equal, and each bisected by two membranes. On each of these boxes are fixed two supports, which sustain copper rods terminating in small claws, in which the electrodes are retained. The pile I employed was made of a few pairs and charged with slightly acidulated water. It is easily conceived why I have chosen this pile; as it is one of the most general consequences we have deduced in this work. The modifications caused in the different parts of the reophorous exercise an influence over the intensity of the current as much greater as the pile is charged with a less active liquid and composed of fewer pairs.

The influence of the inequality of the two positive and negative electrodes may very easily be studied.* We have only to prepare four platina electrodes, in pairs, two of which are more extensive than the other two; the current is then transmitted in the same box by a wide and a narrow electrode. It is only necessary that in one box the positive electrode be wide, the negative narrow, and *vice versâ* in the other box. I employed electrodes of different metals, and varied the relative inequality of the surface of two electrodes, the conductibility of the liquid, the length of the liquid bed, and lastly the force of the pile. The result I am about to describe

* The language I adopt on the progress or direction of the current is that freely adopted by philosophers.

was general and constant. "The electric current is better transmitted when it enters a liquid by the narrow and goes out by the wide electrode, than when the contrary takes place." Under all the circumstances I have stated, and which vary the intensity of the current, these results are always true, but with such modifications as I shall mention. Employing the different metals, platina, silver, copper, pewter, iron, and zinc as electrodes, the law is still verified, but with certain differences which depend on the nature of the metals. In fact we have seen that these different metals, employed as electrodes or diaphragms, cause the currents to suffer a loss, as much less, as they are by their nature, more attached by the liquid conductors. This very easily explains why the difference we are considering is stronger with these metals (used as diaphragms) which cause the current to suffer a greater loss: such is the case with platina. This is indeed what happens by employing platina electrodes in our experiment.

I shall not neglect to mention a very peculiar difference that I have been able to remark with platina only. This metal, in certain solutions, which are generally those of nitric, sulphuric, and muriatic acid, present anomalies to the law we have established: and it is not the greatest conductibility which the liquids we have mentioned possess, that determines these differences; I have several times proved that the law subsists in solutions of muriate of ammonia much better conductor of feeble acid solutions in which the anomalies of platina appear. When we see, for the first time, these phenomena produced with platina, we are surprised at the apparent disorder this metal shows. In the experiments we describe and the solutions mentioned, I hope to throw a little light on the subject. These are the facts in their order: in neutral saline solutions, even concentrated, the phenomenon happens for platina the same as for other metals, whatever liquid conductor be used. When the force of the pile is not very great, the anomalies of platina almost entirely disappear. Solutions of mineral acids are the liquids in which they are most shown. In the solution of sulphuric acid the phenomena takes place in the following manner. I first assured myself there was no electric current between the electrodes alone; for this purpose I had two metallic wires soldered with the claws which held the electrodes and which could lead separately to the wires of the galvanometer. After being certain of the inaction of the electrodes, we made the current of a pile of ten, twenty, and thirty pairs pass, by the two systems, and found constantly that the current is better transmitted by that which has the *narrow*

electrode at the positive and the *wide* at the negative pole. This greater transmissibility does not last long and after a certain number of seconds the needle of the galvanometer repasses from the opposite side. By prolonging the passage of the current, this latter deviation becomes more feeble without ever disappearing entirely. If the electrodes, in other experiments, are tried separately in each system, we find after some seconds of the passage of the current, secondary electricities developed. It now remains, in order to understand the platina anomalies, to discover in which of the two systems the secondary current is most energetically developed. We know by the beautiful experiments of Marianini, that these secondary polarities are developed, with a greater force on the electrode in which the current enters on coming from the liquid, that is to say, on that which communicates with the negative pole of the pile. I also ascertained that the extent of the electrodes has a great influence on the degree of the secondary polarity they acquire. With my pile for constant force of eight pairs, I made the current traverse two electrodes, whose surfaces were in the proportion of a fourth. In one case the current entered the liquid by the wide electrode, in the other by the narrow. The current of the pile passed in the same time; the secondary polarities were, however, very different. When the wide electrode was at the negative pole the secondary current was 40°=75, while it was only 15°=16 when the same electrode was at the positive pole. It appears to me from this, that the phenomena of the platina may be very easily conceived. The system which at first transmits the current best, viz: that which has the wide electrode at the negative pole, is in a little time, reduced at least in appearance to a less transmissibility. The secondary current, which is developed by the passage of the current of the pile and which circulates in a contrary direction to the latter, is much stronger in the system which has the wide electrode at the negative pole than in the other. With the two wires connected to the electrodes, and which could lead separately to the galvanometer, we determined the successive developments of the secondary currents. I however confess that, in certain solutions, I saw even at the first moment of the passage of the current, the transmissibility was greater in the system which had the wide electrode at the positive pole. In these liquids the phenomena are subject to the greatest variations. This superior transmissibility passes from one system to the other, but after a certain time we see it return to the general principle. I do not think it possible, in the present state of our knowledge, to explain this phenomenon; I shall only

remark that these liquids are composed of nitric and muriatic acid, and thereby containing elements liable to attack the platina chemically. I think the phenomena of liquid and gaseous films studied by Messrs. Faraday, Doebereiner, Nobili, Fusinieri, &c. &c., express it for some purpose.

The general principle that we have just established is further proved in another position; it consists of interposing in a liquid trough, a metallic diaphragm, whose two moistened surfaces have an unequal extent. We have also, in this case, a wide plate, by which the current enters the liquid, and a narrow one by which it goes out, as well as the contrary. The experiment is very easily arranged: we have only to take two equal platina plates, almost entirely covering one of their faces with varnish and then fixing them as diaphragms, at the half of the two equal troughs. The two currents pass in such a manner that in one system the positive pole effects the uncovered face, in the other it is the contrary. The general result to be deduced from what we have described is the following: "the current is better transmitted when the uncovered face of the diaphragm is in the cavity of the positive pole than in the opposite case." It must be well remarked that the differences of transmissibility are, under similar circumstances, much less in this second manner of operating than in the former, in which the difference was in the two electrodes. I was convinced of this by employing electrodes equal in extent, at the surface of the interposed diaphragm. I did this in two ways, viz., leaving the electrodes and diaphragm to act in the same direction, or disposing them in a contrary one. The first case is done by holding the uncovered face of the diaphragm in the same cavity in which is placed the narrow positive electrode, equal in extent to the small face of the diaphragm: in the other system the arrangement is the reverse. In the second way the uncovered face of the diaphragm was in the cavity with the positive electrode, and with an extent equal to this same face. With the first arrangement the difference of transmissibility, such as is established in the general law, is found in reality increased, but much less than redoubled. In the second arrangement this difference is enfeebled, but does not disappear, and the excess is produced by the difference of extent of the electrodes.

I think we may easily give a reason for this difference, founded upon principles already established in this work. A second change of conductor weakens the electric current much less than a single and first one.

I must now speak of the modifications to which the established general principle is subjected, produced by the variable relation

which exists between the extent of the electrodes, and the different circumstances which influence the force of the current. Without stopping to notice all the experiments I have made with this view, I shall confine myself to the deduction of the general consequences. I have, already, partly done that in a note extracted from this work, which M. Arago did me the honour to present to the "*Académie des Sciences.*" My ulterior researches have only confirmed and extended them. They are as follows: "the relation between the unequal extent of the two electrodes, which make way for the maximum of transmissibility, increases with the distance to which the two electrodes are immersed in the liquid, and diminishes proportionally to the conductibility of the liquid, and to the degree of force and number of pairs of the pile." Let $a$ be called the surface (+) of the electrode by which the current enters the liquid, $n\,a$ that (—) by which the current leaves the liquid, to obtain the maximum transmissibility, we must change the co-efficient $n$, so that it varies in proportion to the length of the liquid bed, and in an inverse ratio to the conductibility of the liquid reophorous, and the intensity of the current of the pile.

2*d*. SECTION. *Unequal conductibility of the two liquids, traversed successively by the current in a different direction.*

I took two of my varnished boxes, exactly alike, bisected these two boxes with a platina plate. All the remainder of the experiments was made with the same apparatus we described in the first section. The four platina electrodes were equal. Before commencing the experiment I poured the same liquid, distilled or pump water, in the two boxes, and I passed over it the current of a pile of twenty pairs, similar to that I described in the first section.

The needle of the galvanometer would remain immoveable if all things were equal in the two systems. I never commence an experiment without having made this previous observation; then I take some liquid from one of the cavities which is in each box, and pour in these two cavities the same liquid, water a little salted or acidulated, for example. In the two other cavities I pour a less conducting liquid, such as distilled or pump water. After this arrangement, one of the currents proceeds from the bad to the good conductor over the platina plate; and the current in the other box goes in the opposite direction, viz. from the good to the bad.

After a great many experiments, which I think it useless to mention, I obtained the following general result: "the electric current is always better transmitted when it passes

from the bad to the good conductor over a metallic diaphragm, than when it traverses the same system in an opposite direction."

I repeatedly made the two systems, sometimes one and sometimes the other, the better conductor, by changing the bad to a good conducting liquid, with regard to the other, which before, was the good condubtor. This manner of experimenting appears to me much more conclusive, since it admits no other causes than those we have mentioned; and hence it excludes from it every other foreign circumstance.

In making the current more intense, by increasing the number of pairs in the pile, or by its force of production and propagation, I found that the difference of the transmissibility produced by the direction in which the current proceeds, from the good to the bad, or from the bad to the good conductor, is lessened in proportion to the intensity of the current. We have already admitted, in the other section, that the influence of the change produced in the reophorous system, was as much greater as the current was more feeble.

This difference of transmissibility increases generally in proportion to the difference of conductibility of the two liquids which are separated by the platina plate. I also observed that with certain liquids and platina diaphragms, anomalies take place as in the case studied in the preceding section.

If, instead of a platina diaphragm, we use a piece of membrane to bisect the two boxes, the phenomena we have studied is even more clearly verified. Certainly the loss produced by the passage of the current over the diaphragm, is the cause of the difference we remark in using a piece of membrane or a metallic plate. Regarding the effect produced by the pieces of membrane as nothing, and in this case considering the two liquids as touching one another, we may conclude in a general manner; "that the electric current is better transmitted by passing from a bad to a good conducting liquid, than in the opposite case," In each of these experiments I always tried to be certain that there was no electric current produced by the chemical action of the two liquids which met. This phenomena would require a long study to determine all its laws; I purpose returning to it. What I have deduced generally may be expressed in the following manner: "The difference of transmissibility which we study increases, every thing else being the same, with the difference of conductibility of the two liquids which meet, and the first difference changes in a more rapid proportion than the second."

3d. SECTION. *Unequal volume of the liquid at the positive and negative pole.*

I commence by filling with the same liquid, my two boxes, separated in the middle by a piece of membrane. I then pass over it the current, from the pile of twenty pairs, that we have already described, so as to convince me that all was equal in every part. Afterwards, I take from one cavity of each of the boxes, three fourths of the liquid, notwithstanding that the electrodes are entirely immersed in the liquid.

The currents pass in a contrary manner, with regard to the unequal volume of the same box; that is to say, in one box the positive pole is immersed in the shallow liquid: and in the other box the same pole is immersed in all the liquids.

I shall also add a very convenient arrangement for making this experiment: it is by causing the two currents to be compared, to pass through the same liquid contained in two glass tubes bent in the form of U: the two tubes have a bulb at one extremity; the positive wire in one tube is immersed in the bulb in the other in the tube.

The following is the result of a great number of experiments tried with different liquids: "The electric current is always better transmitted when it passes from the smaller liquid mass to the greater, than when the reverse is the case; and that succeeds much better when the force of the pile and conductibility of the liquid reophorous are less.

4th. SECTION. *Unequal losses, according as a diaphragm is near the positive or negative pole.*

I took two of my boxes 25 centimetres square and 1m 50 long. I fixed a platina diaphragm, at a centimetre from the extremity of each of the boxes. The electrodes were placed near the extremities of each box, as in the apparatus we have described in the preceding sections. I filled the two boxes with the same liquid; the current was produced by a pile of thirty pairs. Before commencing the experiment, I caused the currents to traverse the two boxes, keeping the electrodes at the same distance and quite free in the cavities. When the needle remains immoveable I place the electrodes behind the platina plates, but in such a manner that in one box it is the negative which is behind the platina diaphragm, and in the other the positive. The following is the general result which I have obtained: "the electric current is always better transmitted when the metallic diaphragm is near the positive than when near the negative pole," This difference of transmissibility is as much greater as the conditions of the reophorous

favour the loss produced by the diaphragm. A certain length is necessary in the liquid bed to render this difference of transmissibility perceptible.

## 5th. Section.

Speaking of the influence of the volume of the liquid on the degree of transmissibility for the electric current, I remarked that the position of the electrodes at different heights in a liquid mass, was not indifferent with regard to this transmissibility. Without having made a profound study of this phenomenon I am convinced that the current is better transmitted by immersing the electrodes in the lower beds of the liquid than at the surface, and that as far as a certain limit. Hence it is neither in the bed at the bottom, nor in that at the surface, that the conductibility is the best; on the contrary, it is in the middle of the bed.

Proceeding with this fact I have discovered another difference of transmissibility. I placed some electrodes in a quantity of pump water, contained in a glass bell fifteen centimetres wide. The positive electrode was immersed in the bed at the surface, and the negative one at the middle of the liquid. This liquid was thirty centimetres deep. In another perfectly equal quantity I introduced two similar electrodes, but so immersed that the positive was in the middle of the liquid, that is to say, fifteen centimetres from the surface, as was the negative in the other quantity, and the corresponding negative in the bed at the surface. A current of thirty pairs of my pile was passed through the two systems and over the two wires of the galvanometer. The following was the general result: "the transmissibility is greater for the current which has the positive pole at the surface, and the negative in the middle beds, than for the current which has an opposite arrangement."

## 6th. Section.

M. de la Rive has just discovered a fact which completely agrees with those we have mentioned. He has discovered that the electric current is better transmitted when the negative electrode, immersed in the liquid, is heated at the same time that the positive, also immersed in the same liquid, only feels the action of the heat in the contrary case. The heated liquid became the best conductor. Hence the negative pole is immersed in the best and the positive in the bad conductor; this is the case in the second section.

I think it important to add that the facts contained in the third, fourth, and fifth sections of this last chapter may very easily be obtained in another manner. Without making the current of the pile traverse, we employ in the place of platina

reophorous wires, copper or leaden ones; we cut from the same piece of wire, cover it with varnish, so as to leave a similar quantity uncovered. In this case we only employ two wires and the simple galvanometer. Care must be taken to immerse the two wires at the same time. For this purpose they are tied to a glass rod, and this rod is slipped in two vertical grooves. The current which appears in this case is generally that of which the propagation in the liquid is the best. This method is exempt from secondary polarities which are so strongly developed on the platina by the current of the pile, and which renders this sort of experiment so long and troublesome.

The facts I have related in this chapter may be summed up in a single proposition, viz.:

"The electric current which passes over a reophorous partly metallic and partly liquid, is better transmitted when it meets at the positive pole a less conductibility than that which is at the negative pole, than when the disposition of the conductibility at the two poles is the reverse."

I shall conclude this memoir by some very short considerations on the manner in which I think we must regard the electric phenomena. It appears to me that the attention of electricians ought hereafter to be fixed principally on the fact *of the co-existence of the displacement, of the state of vibration and of the motion of the ponderable molecules with all the electrical phenomena.* If we wish for electrical phenomena we have only to put the molecules in vibration, friction, splitting, chemical action, heat, and pressure. This motion, this state of vibration, must necessarily be made in a different manner, in opposite directions, with unequal velocities, according to the density, crystallographic form, relative temperature and direction of the exterior cause of motion between the two bodies we have in view. Hence two different orders of electrical phenomena, which we call positive and negative electricity. The facts of electricity developed on the same bodies, according as they are polished or unpolished, hot or cold, in powder or in mass, rubbed transversely or longitudinally, appear to me very conclusive.

I also mention the fact discovered by Becquerel, of the development of the thermo-electric current, on the same wire by the unequal mass presented to each side of the source, the influence of the state of temper of a wire on the propagation of the current, the development of the current by a single fold of a metallic wire just discovered by Peltier, the relation between the electrical conductibility and the density of bodies, the influence on the electric spark, of the ponderable matters

that it carries and deposits, finally the differences of propagation, according as the conductibility of the middle is modified near the positive or negative pole. I also add, that I can perfectly conceive rotatory motions in the molecules of magnetized bodies, and at the same time an action of removal when they conduct a current; but with electricity, as far as we understand it, I cannot explain these currents round conducting molecules, and at the same time a current which traverses the mass of the body.

They may speak in schools and in treatises of two fluids or of one, but when we work it appears to me well to be disencumbered of them.

## XLIV. *On the production of cold by the voltaic current.* By E. Lenz.

Extracted from the Bulletin Scientifiq. of the St. Petersburg Academy, and communicated by the Author to the Editor of Poggendorff's Annalen. (See Pogg. Annal. xliv. p. 342.)

Peltier was, as we know, the first to remark that cold is generated at the spot where a bar of bismuth and antimony are soldered together by the transmission of a galvanic current when passing from the bismuth to the antimony; but, on the contrary, that heat is evolved when the current flows in the opposite direction.*

The production of cold is a circumstance so much at variance with the other effects of the galvanic battery that, as I learn from private sources, its truth is questioned in many quarters. I therefore think it worth while to establish at once the indisputable accuracy of the fact.†

I began by repeating Peltier's experiments, that is to say, I took a bar of bismuth $4\frac{1}{2}$ inches long, English measure, and 0.4 inch square, and also a similar bar of antimony, placed them across each other at right angles, let them half their thickness into each other at the place where they crossed, and then soldered them together at that spot with tin. Now, calling the ends of the bismuth bar B and B′, and those of the antimony bar A and A′, I first connected B and A with

* See Annales de Chemie et de Phys. Vol. LVI., p. 371. In Peltier's paper however, owing to a misprint, the direction of the current is stated incorrectly.

† The experiments of Professor Moser have already done this, but by other means. See Dove and Moser, Repertor. Vol. I., p. 353. Berlin, 1837.

a multiplier adapted to the indication of thermo-electric currents, and then brought B′ and A′ into connexion with a Wollaston battery of half a square inch surface. When the direction of the galvanic current was from B′ to A′, the multiplier, immediately on the circuit being closed, indicated a declination of 80°, owing to the thermo-electric current called into play at the soldered spot. Afterwards it became stationary at 20°, thus showing, by the direction of the declination, that that place had cooled down. But when the galvanic current passed from A′ to B′ there was caused a deviation of the needle of the multiplier in the opposite direction amounting to 50°, and which became stationary at 30°. On applying to the solder a small thermometer, the bulb of which was 1¾ lines in diameter, there was, in the first case, a diminution of temperature of 1°.75 Fahr., but in the second an increase of 7°.45.

In this experiment one might be led to suppose that a portion of the galvanic current had traversed the thermo-electric chain, thus causing the movement of the needle of the multiplier, in which case, naturally enough, the deviation of the needle of the multiplier would change on the direction of the galvanic current being reversed.

This however is not the case: *First.* Because we can assign no reason why this secondary current should traverse the thermo-electric arrangement more readily in one direction than in another, and this is proved by the multiplier indicating no deviation at all when A and A′ were connected with the galvanic battery, and B B′ with the multiplier, or vice versâ.

*Secondly.* I obtained similar results if I first of all placed A′ and B′ in connexion with the galvanic chain for some time, and then interrupted the communication and brought A and B into connexion with the multiplier, only the deviation of the needle was not so great.

*Thirdly.* The direct indications of the thermometer bear out the truth of the phenomenon.

In order to render this last most direct, and, consequently, most striking proof, more complete, I had two rectangular bars of antimony and bismuth of the dimensions already mentioned soldered to each other so as to make a single bar of double the former length, and I got a hole bored into them at the place where they were soldered, and into this hole the bulb of a small thermometer was inserted reaching half way through the solder. The upper unoccupied portion of this hole was entirely filled up with iron filings. The circuit between a single pair of plates of zinc and platina presenting a square foot surface was then closed with this bar. When the current passed from the bismuth (B) to the antimony (A) the ther-

mometer sank from 59°.6 Fahr. to 53°.15, or above six degrees and a half; after some time, however, it began to rise. This was evidently caused by the bismuth bar becoming considerably heated, so much so indeed as to be sensibly so to the touch, and thus by its conducting power raising the temperature of the soldered part.

The antimony bar did not become sensibly warm to the feel, which, as we shall see in a subsequent article, is due to its superior conducting power. On the current being reversed and made to pass from A to B the temperature rose till it exceeded 140°.

This experiment establishes beyond a doubt the fact of a reduction of temperature when the current flows from B to A. It also shows us that it is a bad plan to lead both metals through the ball of an air thermometer as Peltier does; for with such an arrangement there are no means of preventing a portion of the bismuth bar being also included in the ball with the solder, in which case the heat proceeding from that metal will easily overbalance the refrigatory effect produced at the solder.

To conclude, I have also demonstrated the fact of a diminution of temperature in another manner. I filled the hole in the solder into which the thermometer had been inserted, with water, and laid the bar upon melting snow and covered it therewith all over except where the solder was. The temperature of the bar was of course brought down by that means to 32° as indicated duly by the thermometer when inserted into the water in the hole during the space of ten minutes. And now on causing the current to flow through the bar from B to A the water in the hole in the solder was completely frozen in five minutes. On dipping the thermometer into this water it fell to 24°.12. This experiment, in conducting which Professor Jacobi, of Dorpat, and Dr. Nervander, of Helsingfors, took an active part, was repeated several times, and always with the same result.

This is, as far as I am aware, the first production of ice by galvanism.

*Upon the conducting power of bismuth, antimony, and mercury.*

I have determined the conducting powers of these metals by means of currents of induction, according to the method that I have frequently had recourse to, and of which the description appeared in the thirty-fourth vol. of Poggendorff's Annals; and as two of the results I have thus arrived at, namely, the conducting powers of bismuth and of antimony, are doubtless brought into play in the above experiment, it will not be out

of place to mention them here. The experiments were in the first instance made at the same temperature, namely, when the room was at about 66° Fahr. I had square rods of bismuth and antimony cast of about two feet four inches long, and they were reduced as nearly as possible to the same dimensions by filing, their size being ascertained by measurement. The mercury was poured into a glass tube whose length was accurately known and whose diameter was ascertained by weighing the mercury. Calling the conducting power of copper 100, the results were as follow;

Conducting power of mercury = 4.66
antimony = 8.87
bismuth = 2.58

These results have all the accuracy of my former ones of the same kind.

My next endeavour was to ascertain the conducting power of both bars of bismuth and antimony when forming one bar, first when the current proceeded from the bismuth to the antimony, and afterwards when its course was in the contrary direction. For this purpose I connected the bars together with a piece of copper wire an inch long, the other ends of the bars being brought into connexion with the electromotory coil. I obtained in this manner when the current passed from the bismuth to the antimony a resistance = 3.53. When the current passed from the antimony to the bismuth a resistance = 3.59. The resistance offered by a foot of copper wire whose section is 0.0008856 square inch English is here taken as unity, and this is the normal wire I employ in all my other investigations of a similar nature. It makes therefore but little difference to the conducting power whether the current proceeds from the bismuth to the antimony or the reverse when traversing both bars in connexion. The resistance offered also is very nearly equal to the sum of the resistance of both bars, as determined when not in connexion. This sum is 3.50 and it agrees pretty closely with the mean of the two numbers above given, namely 3.56.

The conducting power of these two metals, however, appears to vary considerably. I observe by reference to my journal that about a year ago I ascertained the resisting power of a thermo-electric system of five pairs of bars of bismuth and antimony to be 3.71, whereas according to the values just given it ought, from the size of the bars employed, to have been 5.04.

The conducting power of a rod of bismuth melted into a glass tube was in the like manner determined to be 1.60 instead of 2.58, the number given above.

The lower value of the former number may however arise from the melted bismuth bar having considerable air bubbles in it in several places, whereas the rectangular bar above examined was a very clean and compact casting.

*Upon the relation existing between electro-magnetic and magneto-electric currents.*

In the paper contained in the thirty-first vol. of Poggendorff's Annals, p. 483, I have worked out the idea that every electro-magnetic experiment can be so reversed as to cause it to bring forth corresponding magneto-electric phenomena. All that is required for this is that the galvanic conductor should have communicated to it by some other means the same motion that it has when traversed by the current during the electro-magnetic experiment. In this case there will be *generated* within it a current whose direction is contrary to the former one. I have in that paper endeavoured to show by means of the most generally known electro-magnetic experiments, that that position is well founded; and very lately I fell in with an interesting confirmation of the views I there laid down.

All those who have occupied themselves with galvano-electric experiments are acquainted with the magneto-electric machine of Pixii in which the whole phenomena of the galvanic current were first produced by the rotation of a magnet. In this machine the current is generated at the first half turn of the magnet in one direction, and in the succeeding half turn in the opposite direction, and so on. To give these contrary currents one and the same direction, Pixii adapted Ampère's commutator to the wire in which the current was generated. I have replaced this commutator by a rotatory one similar in principle to Jacobi's (see Annals vol. I. p. 420), and which is fixed directly into the rotating axis of the steel magnet. By this means I obtain a current, intermitting it is true but passing always in the same direction, and which for instance magnetizes an electro-magnet against which the anchor is held so strongly that it supports a weight of 70lbs.

In accordance with the above mentioned law of the reciprocity of magneto-electric and electro-magnetic phenomena continuous rotation should be obtained (the arrangement of the apparatus being no ways changed and the magnet remaining stationary), as soon as there is called forth by a pile the current imparted in the last experiment by induction, the rotation however will take place in the contrary direction. On performing this experiment in company with Professor Jacobi, with an arrangement of twelve pairs of Wollaston's plates of twelve square inches surface it succeeded completely. The

magnet at first rotated with sufficient force to carry round the wheel work connected with it by a winch. It however in this experiment soon lost the greater part of its original power owing to its approaching too closely the synonymous poles of the anchor when powerfully magnetic. It is easy to see plainly how this rotatory motion must of necessity occur, being in point of fact nothing more than Professor Jacobi's rotating machine in which one of the electro-magnets is replaced by a steel magnet.

JULIAN GUGGSWORTH.

Wormwood Scrubbs,
15th Sept. 1838.

---

## XLV. *An attempt to prove electric phenomena to be dependant upon vibration, from three experiments of* Professor Moll. *By* Mr. Pollock.

Read before the London Electrical Society, September 18, 1838.

In the former papers, read before the Society, I endeavoured to show, that the phenomena of electricity can be explained, if considered as the result of the vibration of matter.

In proof of the existence of such vibration, I relied upon the observation made by La Place, that heat is disengaged by the vibration of the air accompanying the progress of sound through it; believing that such heat can only arise from the fact of the heat disengaged during the compression of the air, not being absorbed during the rarefaction attendant upon the same vibration.

This heat whenever generated by motion through matter appeared to furnish a general test for vibration; not for that accompanying sound merely, but also for the vibration supposed to attend the motion of the electric action through matter, such as for instance, through the voltaic battery. This test, however, has not been considered as worthy of reliance. It is necessary then to look out for further proofs. These appear to be furnished by the phenomena of thermo-electricity.

The two experiments of Professor Moll,* when he applied heat and also cold to the point of junction of antimony and copper, the two dissimilar metals of the thermo-electric apparatus, appear well adapted for this purpose.

* Dr. Seebeck, of Berlin, was the first philosopher who made this experiment.—Edit.

C, fig. 2, Plate XII, is a copper wire, A an antimony bar, S N south and north positions, *n* the magnetic needle, *j k* the positions of junction of the two metals, *v* the direction of the current passing out of the antimony into the copper.

When heat is applied at *j*, the deflection of the needle is eastward, or such as to indicate that a current passes through the arrangement, as shown by the arrows. If cold be applied at *j*, the deflection is westward, and the other phenomena are reversed. Professor Moll also performed a third experiment with a voltaic arrangement, somewhat similar to the above thermo-electric one (see fig. 3), except that zinc, Z, was substituted for the antimony; and acid, A, instead of heat or cold, applied to the junction *j*, of the two metals. This remarkable fact is to be observed, that the currents and the deflection are the same as when in the thermo-electric apparatus, cold and *not* heat is employed.

These experiments are most important from the direct reference they bear to the theory of vibration. Here are two metals employed in this arrangement, antimony and copper: the former is one of the worst of conductors, both of heat and of electricity; and the latter one of the best. Therefore, according to the common notion, the heat on being applied at *j* ought to pass along the best conductor, the copper; and on its arrival at *k*, enter the worst conductor, the antimony, thus constituting a current along the copper from the heated to the cool part. But what says the experiment? It says that so far is this common notion from being true, the very reverse is true; the current actually passing from the worse into the better conductor, as at *k*, thus proving the theory of vibration: the facts being such as according to that theory, they ought to be. The effects of the expansion attendant upon the application of the heat, however limited or partial, are not confined to the point of application, *j*, alone; but are felt at a distance as at *k*, before it is possible the heat itself can have travelled thither.

There is another view of this experiment which bears further evidence to the truth of the theory of vibration. We may put it in the form of a question. Is the heat rendered requisite by the expansion of the copper at *j*, supplied entirely by the lamp, or is it derived from some other additional source? If the lamp furnishes the only supply, then the theory of vibration tumbles to the ground. If not, then is that theory established in all its power. If the heat of the lamp were the only source, then ought the current to pass along the copper from *j* toward *k*; but, as observed before, so far from this being a fact, it is also derived from the antimony at *k*;

where it is impossible the heat of the lamp can have produced any direct influence.

There is a third view of this experiment tending still further to establish this theory, which may be taken. I believe it is agreed by nearly all electricians that all bodies contain either one or two fluids, and it is only when the equilibrium is disturbed, that electrical phenomena are manifested. We may then enquire; can heat be applied to this thermo-electric apparatus, as at *j*, and expansion take place, without the equilibrium of the electric fluid previously existing in it being disturbed, and electrical phenomena being produced? The experiment gives the answer in the negative; and it, moreover, informs us that when the heat has entered the copper, and has equably diffused itself, thereby restoring the equilibrium, the electric effect ceases: thus proving incontestably that the electric phenomena are the result of the influence which the heat exercises upon the fluid previously existing in the apparatus, disturbing its equilibrium; and not of its direct admission. This experiment, therefore, proves vibration, or stages of alternate expansion and contraction, according as the fluid is admitted or emitted. It is incumbent upon the opponents of this theory to show how unequal expansion of a body can take place, without the equilibrium of its fluid being disturbed.

*Recapitulation.* From the foregoing, three things have been attempted to be shown. *First.* That the heat of the lamp generates a current in the copper in a direction contrary to that which, according to common notion, it ought. *Secondly.* That the lamp is not the only source of the heat requisite on the expansion of the copper. *Thirdly.* That the heat of the lamp produces this thermo-electric action, not by its direct heating powers, but by indirectly disturbing the equlibrium of the electric fluid previously existing in the apparatus. It appears impossible to explain these electrical phenomena, except upon the supposition of their being dependant upon matter undergoing clearly defined stages of vibration. *Moreover,* from the above investigation, it is scarcely possible to avoid the conclusion, that what is termed the latent heat of bodies, and their electric fluid, are identical.

If electricity be dependant upon the vibration of matter, it is of the first importance to be satisfied of the truth of that theory, and to apply to it further examination.

When we examine the nature of force and resistance, the result will be found in strict accordance with that theory; thus, the electric current cannot have an uniform velocity, either by that law or that theory.

insulated, metallic cylindrical segments, secured on the shaft, and two stationary metallic tangent springs for conductors. Silver, about the purity of coin, answers best. The wires from each of the armatures, pass through holes in the brass straps, and through openings in the sides of the machine, to be attached to the pole changers, one of which is seen at *h*. One pole changer would suffice for both armatures, but by using two, the experiments may be considerably varied, as the separate coils may be combined, to form a simple or compound battery. *i i*, are the tangent springs of copper, but tipped with silver where they rest upon the silver pole changer. They pass up through the top board of the machine, and are soldered respectively to the brass straps *k k*, into which are screwed the mercury cups, one of which is of glass for exhibiting the spark, combustion of ether, alcohol, oil, &c. These cups represent the two constant poles of the revolving coils. The circuit is completed and broken by the rise and fall of the curved wire *m*, attached to the little lever *l*. At *l*, the lever is pulled down by a spiral spring. At the other end is fastened a string, which passes down to the lower lever, *n*. This is worked by the revolving pins *n s*, attached to a moveable ferrule on the shaft. The pins *n s*, are themselves binding screws, so that the ferrule may be adjusted in any position, and the circuit broken at any required time. This electrotome, as it may be called, is removed when decompositions are performed, and the platinum wires are inserted into the mercury cups. The machine works equally well, turned either way. I have not yet had an opportunity to measure the rate of decomposition of pure water, but it is certainly as rapid as from one hundred pairs of galvanic plates. Pure water is rapidly decomposed when the platinum wires are ten inches apart. A few turns of the wheel furnish mixed gases enough to make a smart explosion. A solution of sulphate of soda in cabbage water, contained in separate glass cells, submitted to the action of the machine, gives immediately the characteristic changes of colour by decomposition; reversing the motion of the wheel, the reverse change takes place. The light from charcoal points is insupportable. Plumbago gives an intense light. The metallic leaves burn with splendour. When a wire is suddenly withdrawn from one of the poles or brass straps, a bright spark is obtained, *half an inch* in length. When the circuit is broken from a smooth and clean metallic surface, an entirely new and beautiful appearance presents itself. A diffuse, irregular, nebulous spark, darts along the surface, sometimes in several directions at once, to the distance of half or three quarters of an inch. It suc-

ceeds with all the metals yet tried, but best with a piece of iron finely striated with a smooth file. When the lever trip is worked, the secondary current frequently plays with an intense green light, across the separations between the pole changers. The shock from the direct current is uncomfortable with dry hands; but when the secondary current is used, it is painful to touch the poles, even with the fingers. It causes the gold leaves of the electroscope to diverge strongly, without the aid of a leyden jar, or extended insulated metallic surfaces. It charges a leyden jar at every touch. A charcoal point on the knob of the jar, affords a bright light at every touch. I have some time since shown, that the shocks and decompositions produced by the secondary current of flat spirals and helices in connexion with a single pair of plates, were greatly increased, if the surface of the mercury, (or solid conductor,) from which the circuit is broken, be covered with pure water or naphtha. I have since found that oil gives a far greater increase than either. The rationale is now obvious. When the battery circuit is completed, (as shown by Faraday's discoveries of volta-electric induction,) a feeble secondary current flows in a direction opposite to that of the battery. When the circuit is broken, a powerful secondary rushes in the same direction with the primitive battery current. Hence the bright spark is occasioned by the passage of the secondary through the heated air, occasioned by the combustion of the mercury. Now, if the surface of the metal is covered with a non-conducting liquid, such as oil, the circuit is broken with precision, while an obstacle is offered to the consequent and secondary current, and the greater part of it rushes through the body, or whatever conductor joins the extremities of the coil. The application of this fact is of great value in the use of the magneto-electric machine. If a drop of oil be put upon the break piece of the ingenious machine of Clarke, its power will be greatly increased, while it preserves a good contact by saving the metals from oxidation. I find also, that if the stratum of oil be very thin, the spark is more brilliant than without it, being partly due to the combustion of the oil. The same is true also of charcoal points, when used with the deflagrator or magnetic electrical machine. It will be seen at once, that the gain of power in this new magnetic electrical machine must be very great. I notice in the last No. of this Journal, that Clarke's machine has on one armature four thousand five hundred feet of wire, whereas on one armature of this machine there are only eight hundred feet. The source of this superior action, is chiefly the use of the straight armature, instead of the revolving horse-shoe. There is no advantage in covering

a horse-shoe (for electro-magnetic or magneto-electric purposes) beyond the straight portion. Hence the piece of iron is longer than necessary for the full and ready production, and neutralization of the magnetic forces. The straight armature is covered through its entire length, and covered with ease and precision in a lathe.

*On a new compound electro-magnet, for the production of the magnetic electrical spark, and also for attractive force.* The following positions, I think, may be considered as well established by experiment. 1st. Very long and large bars of soft iron, even with a proportionate battery, acquire a comparative feebler magnetic intensity than smaller bars. 2d. That long and large bars of soft iron, once charged by a battery, retain a greater degree of magnetic power than smaller ones. The great magnet lately constructed by Dr. King of this city, (Boston) in imitation of Prof. Callan's magnet, affords convincing proof of these facts. This magnet is made from a bar of two and one half inch iron, and fourteen feet long. Its lifting power is about one thousand five hundred pounds, and when the battery and armature are removed, it retains a permanent lifting power of fifty pounds. Struck with this curious fact, I was led to the construction of the compound electro-magnet. My first experiment was made with three separate layers of coiled wire round a wooden spool ten inches long, with a bar of soft iron enclosed. This was connected with two pairs of plates and the spark and shock observed. The bar of iron was then removed, and a bundle of annealed large iron wire, introduced in its place. The sparks and shocks were increased to a surprising degree. I then took a bundle of smaller wires four inches long, and wound them with only two layers of continuous wire. The spark from this was as intense as that given by the large bar in the spool. I next took seven pieces of good hoop iron, well annealed, one inch wide, one fourteenth of an inch thick, and eight inches long. These were firmly rivetted together, and the angles of the compound bar thus made, were rounded to prevent cutting the wire. Four layers of coiled wire were then wound upon it, and their ends attached to two connecting wires. Nothing can be imagined more intense and beautiful than the sparks produced by this little compound magnet. When a piece of iron is burned with it, the ignited particles are frequently thrown off, the distance of two feet, and occasionally fall to the floor. For its size, it is the strongest electro-magnet I have ever known, and when the battery is withdrawn, there is not magnetism enough retained, to affect a very delicate needle. From this perfect neutralization of

power, arises in great measure, the intensity of the secondary current. The neutrality is partly due to the reduced size of the bars, but chiefly to the action of their similar poles upon each other, when the exciting cause is withdrawn.*

Fig. 6, represents a small electro-magnetic bar, mounted for rotation, with my pole changer attached, which is shown at *a*. *b b*, are the conducting tangent springs; *c c*, mercury cups for connexion with the battery. No stationary magnets are here used, the instrument being made with such delicacy as to revolve by the earth's magnetism. Placed upon the top board of the machine, the connexions being made, it revolves rapidly by the attractions and repulsions of the upper poles of the large magnets underneath.

A small electro-magnet, charged by the three inner coils of one of the armatures, sustained permanently ten pounds, while the machine was in action.

*Fusion of iron filings.*—When a wire from one pole of the machine was placed in a heap of fine iron filings, and a bunch of these raised by a magnetic bar connected with the other pole, the connecting shreds of filings, sometimes an inch in length, became intensely ignited throughout, fused into a mass and fell off, leaving frequently a globule attached to one of the poles, which glowed for a time after the contact was broken, and then exploded, as is seen with particles of iron burning in oxygen. This curious experiment succeeds best, when the filings are held between two opposite poles of magnetic bars, connected each with the poles of the battery.

---

## XLVII. *Facts and observations for the purpose of illustrating a Theory intended to connect the Operations of Nature, upon general principles.* By Paul Cooper, Esq.

(*Continued from page* 313.)

132. The last of these experiments is entitled to attention from its pointing out the relative electrical forces of potassa, sulphur, and copper. To produce the decomposition and recomposition in the manner described, the electrolyte must

* The explanation here given by Dr. Page of the almost total disappearance of polarity when a bundle of wires or strips of iron are employed in the helix, appears to us, to be perfectly satisfactory. For a complete investigation of the effects of different forms of soft iron, when subjected to the action of electrical currents, see p. 470 of Vol. I. of these Annals. Edit.

be arranged as in figure 78, Plate X.; in which it will be observed that the sulphur is united to the potassa by a positive surface, a proof that sulphur is positive to potassa; and likewise that the sulphur must acquire a positive surface to enable it to unite to the copper, a proof that sulphur is also positive to copper (102). It is probable that potassa is negative to copper, from its inducing a positive surface in the iron in opposition to the natural inductive force of the copper. In this experiment potassa appears to bear the same relation to copper, that in former experiments hydrogen bears to zinc (86).

133. An erroneous view of the character of the electro-motive forces previously described (87), which, from want of due consideration, led me to suppose that the direction of the current arose out of the balance of two opposing currents, instead of a balance of electro-motive force upon every surface in the circuit, produced by the inductive influence of the elements upon each other (144), induced me to adopt the opinion (in opposition to views I had long previously entertained*), that the increase of the electro-motive forces in associated voltaic circles was derived from a concurrence of the forces in the different cells of the battery; which, by acting in union, produced a joint force. (See Researches 989, 990.) But further consideration has satisfied me that there are no means of connecting the forces in this manner, and that we must look for this increase of force to the increased intensity of derangement in the plates, produced by their action upon each other (141). If we could get over this difficulty by supposing the forces in the different cells to be united by drawing or propelling the current in the same direction, there would be another objection to the explanation; we should have two causes to produce one effect, either of which would be sufficient for the purpose, for there must be increased intensity (136) in plates thus associated, and this increased intensity must produce in every part of the circuit an increase of electro-motive force.

134. That increased intensity of derangement is the result of associated numbers, will be evident from a consideration of its effect, in giving the shock to any part of the animal system which is included in the circuit. It is well known that the force of a shock increases with the increased number of plates, quite independent of their size; it is therefore neither the quantity of light nor the velocity of its transmission that produces the effect, for both these are dependant on the size of the plates: the quantity transmitted must

* (See Abstract 18.)

necessarily influence the velocity of its transmission, because a slender wire usually forms part of the medium by which the circuit is completed, whatever may be the size of the plates. As neither the quantity of light nor its velocity have any influence with regard to the intensity of the shock, it is reasonable to conclude that its transmission is not the cause of it; and, consequently, that it must result from the communication of the derangement of the plates to those parts of the system which are included in the circuit. The communication of this derangement from atom to atom through all intervening bodies which form parts of the current is an essential part of our theory; and as we find the force of the shock is increased by increasing the number of plates, we may conclude that the intensity of their derangement, is increased by the same means, it being obvious that if the shock be dependant upon the communication of derangement, its force must be in proportion to the intensity of that derangement. The same conclusion may be drawn from the greater *extension* of the shock, when the body forms part of the circuit of an increased number of plates.

135. My views with regard to the increased intensity derived from associated voltaic circles will be further explained by applying the principles of my theory to the experiments of Dr. Faraday on interpositions.

"The reversal, by accident or otherwise, of the plates in a battery has an exceedingly injurious effect. It is not merely the counteraction of the current which the reversed plates can produce, but their effect also in retarding even as indifferent plates, and requiring decomposition to be effected upon their surface, in accordance with the course of the current, before the latter can pass. They oppose the current, therefore, in the first place, as platina interposed plates would do; and to this they add a force of opposition as counter voltaic plates. I find that, in a series of four pairs of zinc and platina plates in dilute sulphuric acid, if one pair be reversed, it very nearly neutralizes the power of the whole." (Researches 1046.)

136. I shall commence the subject with this experiment, because it is easier to trace the cause of opposition to the passage of the current from a pair of plates reversed, than from the interposition of platina plates only. When plates of zinc and platina are brought into metallic contact, the zinc presents a positive polarity to the negative polarity of the platina upon the surfaces in contact; and upon the opposite, or exposed surfaces, the zinc presents a negative and the platina a positive polarity (49). The polarity thus induced arises from the difference of electrical force in the two metals,

and the derangement of the atmospheres of the atoms of the metals, which produces the polarity, increases with the increase of this difference. There is always a transfer of light from the negative to the positive metal upon bringing the two metals into contact (62); and the force with which the transfer is made also increases with the increased difference of the electrical forces. The force with which this transfer is made I have called the electro-motive force (59); and I propose to distinguish the comparative state of derangement arising from the contact of different bodies by the term intensity. Thus the intensity of the polar forces, both positive and negative, will rise with the increased difference of the electrical forces of the bodies which produce them.

137. Dr. Faraday found that in a series of four pairs of zinc and platina plates, if one pair be reversed, as in fig. 79, it very nearly neutralizes the power of the whole. If we

the three pairs of plates which are placed in
er, it will be evident that no polar arrangement
te can take place, because the forces will be
in both directions; in one of the cells both
zinc and in the other both will be platina;
rill be connected, one pair by actual contact
and the other by the wire A B, so as to produce an equality of forces. If we replace one of the pairs of zinc and platina
eir proper order, the electrolytic arrangement will
n the first cell, and, perhaps, in the second; but
hird, because the inductive forces of the two pairs
ll not have sufficient power to give a polarity to the
ir in a direction the reverse of that which would be
y their own forces, and which is necessary to pro-
an arrangement. It will be observed that the inductive forces of the two pairs of plates must be conducted by the electrolyte, and it is reasonable to expect that their forces, thus conducted, will not be more than sufficient to counteract the inductive forces of the reversed pair so as to bring the plates to a natural state (116).

138. Under these circumstances there will probably be a trifling current, with the usual decomposition, from the first zinc plate to the platina of the second pair of plates, and from the zinc of this pair, with decomposition if the fluid has been electrolytically arranged, to the zinc of the reversed pair; but as the polar arrangement will cease here, it must be conducted by these plates and the by fluid in the third cell, as metals and fluids conduct the electric fluid under ordinary circumstances. There will, however, be some degree of derangement communicated to the fluid in the third cell by the end zinc

plate, and the current, thus impeded, will be extremely feeble. When a continuous current makes its appearance, it is a sufficient proof of an electrolytic arrangement, followed by chemical action in some part of the circuit; for *local* circuits produce no *general* current. (See Researches 947, 996.)

139. If we now replace the other pair of zinc and platina plates, and thus restore the battery to the state represented in fig. 79, the inductive force of the three pairs of plates in their proper order will have sufficient power to c natural forces of the reversed pair, and produ arrangement upon their surfaces indicated in the usual positive and negative signs. Dr. Farad one reversed pair *nearly* neutralizes the other which I infer that a weak electrolytic current wa under these circumstances throughout the ci could not have been the case without the polar described in the figure.

140. The negative state of the surface of the platina of the reversed pair, presented to the fluid in the fourth cell, is not only necessary to produce an electrolytic arrangement, but it is also necessary for the preservation of this arrangement; for admitting its formation to be possible while the platina presented a positive, or even a neutral surface (137), upon its conducting the current to the oxygen, the two surfaces would repel each other before the oxygen could r quantity of light to reverse its poles. In the adhesion of the positive element to th it is receiving from it the current which poles, it is essential that the surface of the e may be its natural character, should presen corresponding with the positive state of which it is in contact.

141. It being obvious, then, that the inductive forces of the three pairs of plates, placed in their proper order, have sufficient power, when conducted by the electrolyte, to produce a polar arrangement in the reversed pair in direct opposition to their natural forces, it is equally evident that if this pair had been placed in the same order as the other three, its natural forces would have received an increased intensity from the operation of the same forces conducted by the same medium. The inductive forces of the first and second pairs of plates could only have been conveyed to the fourth through the medium of the third, and, consequently, the third pair must have had its intensity increased by the inductive influence of the second, the force of which was previously augmented by the similar action of the first. But the reversed order of the

fourth pair of plates would not only produce a resistance to the progress of the inductive forces of the other three, it would also, by, reaction, lessen the intensity of these forces; first by its action upon the third, and through the medium of the third upon the second, and, in like manner, upon the first. The increased intensity of the second and third pairs of plates would upon the same principle react upon and increase the intensity of the first; and if the forces were conducted metallically instead of electrolytically, the circuit being completed, the whole series, notwithstanding the reversed order of the fourth pair of plates, would probably be brought into nearly an equal state of intensity.

142. We may further observe, that the inductive forces of the plates being electrolytically conveyed from one to the other in this arrangement, the electrolyte must have its natural forces increased if they correspond with the force which produces the current, and reversed if, like the reversed plates, its forces are in a contrary order, before they can be prepared to communicate these forces. Now the natural forces of the particles of water which form the electrolyte are in the reverse order to the forces which give direction to the current; and, consequently, these forces, like the forces of the reversed plates, must be reversed before they can become conductors of the inductive forces from one pair of plates to the pair next in succession.

143. It is extremely difficult to describe the conduction of the deranging or inductive force, independently of the current which is necessary to complete it (119); but the state of tension produced by this force in its incomplete state, must be conveyed through every part of the circuit before the light can be transferred from one body to another in any part of it, because the transfers throughout are necessarily simultaneous.

The full development of this subject would lead us to some very curious and interesting results. It will be evident upon an inspection of figure 79, that the electrolytic arrangement there described, must be made in the whole of the cells before the current can pass; but there can be no motive for this arrangement in the fourth cell before the reversed pair of plates are brought into the polar state in which they are represented by the inductive influence of the other three. Now, this inductive influence can only be transmitted through the medium of the electrolyte, and the electrolyte itself, previous to the reversal of its polar forces, is in the same reverse order as the fourth pair of plates; yet it is found that the force is transmitted, because we know, from the effects produced, that an electrolytic arrangement is made throughout the cir-

cuit. The inference we must necessarily draw from this, is, that the state of tension produced by the propagation of the inductive force, is equivalent in its results to what might be expected after its actual completion; that, in fact, it is transmitted from one body to another, previously to any change of arrangement in the transmitting medium, precisely as if that change were actually made, which can only be the final result. Thus, although the electrolyte in the cells of figure 79 must retain the present arrangement until the current can pass to reverse the polar forces, the inductive forces of the three pairs of plates in their proper order, produces a state of tension constantly exerting itself to give a positive surface to the oxygen in contact with the zinc, and a negative surface to the hydrogen in contact with the platina; and, although this state cannot be completed before the passing of the current, the hydrogen in the third cell acts upon the zinc of the reversed pair of plates, by inducing a positive state of tension upon its surface in opposition to the force produced by the platina, precisely as if it were in the full possession of that negative force which can only be the result of a general transfer of light throughout the circuit.

A little consideration will satisfy us that the inductive force is propagated independently of the means necessary to complete it (119); for induction being an action of contiguous atoms only, it follows, that its force upon any other condition could not be extended beyond a single section of atoms unless the derangement of this section were first completed by the necessary transfer of light; whereas we know numerous instances in which it is thus transmitted to considerable distances. When Van Marum charged his battery by connecting it with the voltaic pile, the state of tension produced by the inductive force of the pile, was transmitted to the whole of both its surfaces by slender wires; and the transfer of light necessary to complete the derangement was made from one surface of the battery to the other through the medium of the pile as a conductor. The transfer in this case was evidently the consequence of a state of tension arising from the previous propagation of the inductive force. "In this pile, consisting of silver, zinc, and moist cloth, the silver being the lowermost of the metals, the electricity was positive at top, and the electricity communicated to the interior surface of the battery was the same the contact being made with the zinc at the top of the pile."*

* Nicholson's Journal, Vol. 1, Octavo Series, page 173.

In the arrangement here described, the current upon completing the circuit must have been from the silver to the zinc (see figure 79); but this would have had a contrary tendency to the effect produced by it, the silver being in communication with the exterior and the zinc with the interior of the battery. If, however, we consider the effect as arising from the propagation of the inductive forces of the pile, the result was what might have been expected, the zinc exhibiting its positive surface at the top of the pile and the silver its negative surface at the bottom.

When a spiral form is given to the polar surfaces of a wire, under the influence of a galvanic arrangement, to enable it to become a conductor, the inductive arrangement must be made throughout the wire previously to the transfer of a single atom of light; for the instant that an atom of this fluid is received at one end of the wire an atom must be delivered at the opposite end (68); to effect which, there must be a current through its whole length, which will require a connected arrangement.*

The same inductive preparation is required for the transmission of the light of the sun or light from any other source, under similar circumstances of derangement, as for electrical light; and it is even doubtful whether under any circumstances the media by which light is transmitted are in a proper state for this purpose without some such preparation.

144. It appears, then, that bodies in a voltaic circuit act and react upon each other so as to control the natural forces of the different bodies and produce an electro-motive force upon each surface throughout the circuit in the same direction. If, for instance, the natural electro-motive force of zinc to oxygen be five, and the natural force of hydrogen to oxygen six, these forces are controlled in such a manner by the inductive forces of the platina and the other bodies in the circuit with which they are associated, that the natural forces, and with them the natural affinities of the bodies, become reversed; the electro-motive force of the surface of the zinc, in consequence of the negative state induced upon it, becomes exalted, say to six, and the electro-motive force of the hydrogen, in consequence of a less negative state being induced upon the surface it presents to the oxygen, reduced, say to five, leaving a balance in favour of a current in a direction the reverse of that which would be produced by the independent action of their natural forces (See fig. 79). Again, the natural electro-motive force in the reversed pair of plates is from the platina

* See a paper on the communication of Magnetism &c. Annals of Electricity, &c., Vol. I. p. 230.

to the zinc; but the induced force, in consequence of the platina presenting a positive surface to a negative surface of the zinc, is from the zinc to the platina (131); the current, therefore, is not opposed by any actual resistance from bodies thus interposed, but they lessen the electro-motive force of every other body in the circuit by their reaction.

145. The opposition to the current will be of precisely the same character as in the reversed plates in fig. 79, if the wire A B be broken, and a fluid interposed between the points A and B, with its elements in the reversed order *essential to electrolytic action.* In both cases the loss of force in the current will proceed from the reaction of the elements in the reverse order, upon those which are placed in their natural order, or alternately *positive and negative*; and the amount of this loss will increase with the increased difference of electrical force in the elements reversed (114).

146. Now, under these circumstances, it becomes a question, whether the quantity of light transferred by the current from one surface to the other, will be the same throughout the circuit? It appears that each body has an independent electro-motive force, compounded of its natural forces, and the inductive forces of the bodies with which it is connected; and we know from other experiments, that the quantity of light transferred depends upon the relative state of the two surfaces in contact (136). When, for instance, galvanic circles are formed with plates of platina and zinc, and with plates of tin and lead, the transfer of light between the plates, and, consequently, the general current, is much greater in the former than in the latter. If, then, the tin and lead plates were placed in a battery with the platina and zinc, although the electro-motive forces of the former would be exalted at the expense of the latter, would the transfer of light be the same between surfaces, the natural electrical forces of which are so very different?

147. If this question should be answered in the affirmative, and I am undecided upon the point myself, the equality of the transfer must arise from the increased polar intensity of the tin and lead and the decreased polar intensity of the platina and zinc, in consequence of their being associated in the same circuit; and it will be no proof that their natural polar forces are equal. So, if water be decomposed in one part of a circuit, and a different electrolyte in another, the decomposition of an equal number of particles of the two electrolytes will be no proof that their natural polar forces are equal; it will only show that they have been brought to this state of equality by their inductive action upon each other.

148. If a negative answer to the question should be thought more reasonable (143), the transfer of light between every two surfaces in contact, throughout the circuit, must be regulated by the actual state of their surfaces under the circumstances, and subjected to the various forces to which they are exposed; and the equality of the number of particles of different electrolytes decomposed in the same circuit, will still be no proof that their natural polar forces have equal intensities.

149. No part of Dr. Faraday's experimental investigations have appeared at variance with my theory, or presented any other difficulties than those of mere detail, with the exception of those by which he has endeavoured to establish the doctrine of the definite chemical action of electricity; and, apparently, with so much success. My theory supposes that the polar forces by which atoms are cohesively united into particles and particles into masses, are produced by the difference of the electrical forces of the surfaces of these bodies when in contact; and that the intensity of these forces increases with the increased difference in the electrical state of the bodies which are thus brought into action (136). It necessarily follows, that a transfer of a greater quantity of light will be required to reverse the poles of atoms, the polar forces of which have been produced by the action of surfaces varying greatly from each other in their electrical states, than to reverse the poles of atoms formed by surfaces which in this respect much more nearly approach to each other. The difference, therefore, between my theoretical views, and the inferences which may be fairly drawn from Dr. Faraday's experiments, involves a general principle of considerable importance.

150. The conclusion to which Dr. Faraday appears to have been led by these experiments, that a definite portion of the electric current is required to effect the same object upon an equal number of the atoms of bodies indiscriminately, (Researches 806), is altogether at variance with his more general views. He says, "from the period when electro-chemical decomposition was first effected to the present time, it has been a remark, that those elements which, in the ordinary phenomena of chemical affinity, were the most directly opposed to each other, and combined with the greatest attractive force, were those which were the most readily evolved at the opposite extremities of the decomposing bodies (Researches 669). "The various oxides, chlorides, iodides, and salts which I have shown are decomposable by the electric current when in the liquid state, under the same general law with water, illustrate in an equally striking manner the activity,

in such decompositions of elements directly and powerfully opposed to each other by their chemical relations (Researches 672)." "On the other hand bodies dependent on weak affinities very rarely give way (Researches 673)." "The identity of the force constituting the voltaic current or electrolytic agent, with that which holds the elements of electrolytes together, or in other words with chemical affinity, seemed to indicate that the electricity of the pile itself was merely a mode of exertion, or exhibition, or existence of *true chemical action*, or rather of its cause (Researches 877)."

151. Now, if the elements of bodies be held together by the same agent which constitutes the voltaic current, "whether it consist of a fluid or fluids, or of vibrations of an ether, or any other kind or condition of matter (Researches 994)," and if equal quantities of this agent be interposed between the atoms of all bodies, how is it that they are held together by such very different forces? In what manner are the elements of bodies more opposed to each other in one case than in another; and in what consists the difference between powerful and weak affinities? Again, if this agent be distributed in equal quantities in bodies generally, how is it that bodies the elements of which combine with the greatest attractive force are the most ready to bring it into action? If it should be said, in answer to these questions, that the electrolytic agent has less attraction for the elements of some bodies than for the elements of others, and, consequently, that such elements have less affinity for each other; it may be asked, how is it that with less attraction, it combines in equal quantities and exhibits greater reluctance to a separation?

It appears to me, then, that we can only reconcile these discrepancies upon some such principles as those I have here advanced; which at the same time that they bring Dr. Faraday's experiments and general views into accordance with each other, remove every difficulty which the former previously presented to the application of my own theory.

*(To be continued.)*

---

## XLVIII. *On the supposed influence which the roughness and polish of surfaces exercise over the emissive power of the bodies.* By M. Melloni.*

When we measure the intensity of the calorific radiation which proceeds from the two sides of a metal vase, filled with

* From the Compte Rendu Hebdomadair des Séances de l'Académie des Sciences, August 6, 1838. Translated by Mr. J. H. Lang.

boiling water, having one of its longitudinal halves bright and well polished, the other polished first and then scratched with emery, a graver, or lime, we find the quantity of heat given out by the unpolished or scratched surface, is always more than that of the bright surface: these variations sometimes exceed the proportion of two to one. Hence, it follows that the augmentation observed proceeds from the inequalities themselves impressed on the side of the recipient, and, consequently, that the superficial asperities of the bodies have the property of facilitating the escape of the heat they contain. I am about to have the honour of communicating to the Academy, an extract of a series of researches from which it appears to me evident that this proposition is quite erroneous; so that if the nature of the superficial beds contributes very certainly to vary the quantity of heat emitted by a hot body, the state of the surface has no part in the production of the phenomenon. In the first place I must confess that, notwithstanding the authority of great names, I have always thought the influence of the polish over the calorific emission very doubtful. It is said, the interior heat experiences the same action of surface in quitting the body as it undergoes in penetrating it, as regards radiation; be it so: but why these small *reflecting facets* which are produced by scratching the plate, ought they to reflect interiorly less heat than the *polished* surface *of a single piece?* Take a recipient of yellow copper, having two polished faces, slightly tarnished by exposure to the air; on one of these faces make a series of parallel scratches with a graver, the traces thus produced will certainly be more brilliant than the rest of the vase: and, notwithstanding, the surface furrowed by the graver will emit more heat than the smooth surface. It is nearly two years since I took a part in this objection and some other experiments of the same kind, by M. M. Bache, Henry, and Locke, very distinguished philosophical professors of the United States, who were then at Paris. Now, that the question appears to me very decided, I lay aside indirect objections, and pass immediately to the explanation of the results which lead directly to the proof of the fact I have advanced.

I took a cubic vessel of copper, whose four side faces were very straight; on their exterior angles and bottom edges I had small spring grooves soldered so as to be able to keep plates of two or three lignes thick close against the vessel: then having procured two pair of plates, one of jet and the other of ivory, I applied them to the four sides. Each pair was composed of plates perfectly equal in every thing, except the state of the exterior surface, of which one was very smooth

and bright, the other unpolished and scratched with emery. By measuring exactly with the thermo-multiplier, the quantities of heat emitted by the two polished faces, when the recipient was full of hot water, and comparing them with those which proceed from the corresponding rough ones, I could only perceive a difference of one or two centièmes, and sometimes on one side, sometimes on the other: the mean of twenty observations gave a variation which scarcely amounted to some millièmes, and which was, consequently, too insignificant to notice.

In objection to this experiment it might be said that, notwithstanding the precautions taken to establish the contact between the plates and the vessel, we could not be certain that the two plates composing each of the pairs subjected to proof, possessed the same temperatu
this objection, I had a cubic recipient, cut out
of marble, whose sides, reduced to a perfectly
were differently wrought on their exterior surfa
smooth and brilliant; the second similarly s
and unpolished; the third scratched in one si
and the fourth in two perpendicular directio
filled with hot water emitted from the four sides the same quantity of radiating heat.

Hence, it appears that the more or less irregular state of the *surface* has no influence over the emissive power, when the radiating body is not of a metallic nature.

I covered with smoke black one of the faces of my marble vase, as well as one of the plates of each pair employed in the preceding experiment. By fixing on 100 to represent the emissive power of smoke black, I could easily determine, by successive comparisons, the proportional numbers which represent the emissive powers of ivory, jet, and marble: all three were found comprised between 93 and 98. May we not say, that if in the substances we have just employed the influence of the want of polish is nothing, it is because their emissive power is at the limit of the maximum, where an augmentation could scarcely take place, because the emissive surface no longer makes any hindrance to the escape of the heat; whilst, in the metals, very far from this limit, the alteration of the state of the surface must necessarily exercise all its influence, and render it sensible by a great variation in the quantity of heat emitted.

Although this reasoning be founded on a pure hypothesis, viz., that the black of smoke offers no resistance to the radiation of the surface: and that, besides, the emissive powers of the three substances employed be, on the one hand, far enough

from 100 to admit of the variations produced being appreciated, and on the other so energetic, that the least proportion of a change happening in their values would make them pass over the whole distance, which separates them from this number; however, we will abandon the non-metallic substances for the present, and endeavour to resolve the question with the bodies themselves from which it takes its point of departure.

Copper, zinc, pewter, and tin, which to my knowledge, are the only metals hitherto employed in the experiment described in the beginning, being exposed to the action of the air were quickly covered with a light coat of invisible oxide, the presence of which is, however, deduced in a very plausible manner from certain electrical phenomena. But, we know, the emissive power is much greater for oxide than for metals. Hence, it might be that the rough surface, presenting more points of contact to the air, is more perfectly oxidized than the smooth one, and thus its radiating power would be increased by the single fact of oxidation, without the more or less regular arrangement of the superficial points taking any direct part in it.

In order to see if this explanation be supportable, we have only to operate on gold and platinum, this also I have done; but the scratched gold and platina plates have always a much more abundant calorific emission than the polished ones of either metal.

Oxidation, as well as the influence of the polish in non-metallic substances, being discarded, what is the alteration peculiar to metals which in these bodies might accompany the more or less extensive overthrow of the superficial bed?

No other, in my opinion, than a change of hardness or density. In fact, jet, ivory, and marble, are substances almost entirely without compressibility, or at least they do not possess in a sensible manner the property of retaining, in a stable manner, the modifications of density and hardness which might be imprinted on them by the action of a mechanic force: they form themselves into flakes without being submitted to any pressure. Metals, on the contrary, are compressible and the ordinary plates found in commerce are obtained, as is known, by subjecting the metallic matter to extreme pressure, by means of the hammer and flattner. Experience proves that these plates as well as wires, are, in specific weight and hardness, superior to those of cast metal. Does any one say that this augmentation of hardness and density is uniformly distributed over all points of the mass? Is it not, on the contrary, more probable that during the operation of flattening, the surface has a greater pressure and condensa-

tion than any other part, and the plate resulting from it is in fact surrounded by a kind of crust of a greater hardness and density than those of the interior beds?

That settled, it is evident that by scratching the surface of the plate we shall discover less dense or softer parts. But looking at the tables which represent the emissive powers of bodies, we easily perceive that these powers follow generally an inverse proportion to the densities. Let us admit, by analogy, that the same law is observed on the different states of condensation of the same substance, and we shall conclude from it that by cutting furrows on the surface of the plate we should obtain an augmentation of radiating power. We may add that the parts which compose the superficial bed being disengaged by the subdivision of their mutual contrast ought to extend themselves, and thus acquire, by the diminution of density, an emissive power approaching nearer that of the more tender beds of the interior.

This being the case, it should follow. *First.* That a polished metallic plate radiates a greater quantity of heat in proportion as the density or hardness of its superficial beds is less. *Second.* That in this case of less density or hardness, the augmentation of radiating faculty produced by the want of polish, will be inferior to what we obtain when the plate is more dense or more hard beaten.

It is almost useless to add, that in order to verify these theoretical consequences it is not necessary to employ an oxidable metal at a low temperature: for a plate constructed with these sorts of metals has a tendency to increase its emissive power, which continually varies with the state of the superficial beds, and so much the more as these beds are softer and more divided.

A strong percussion and a slow passage to the solid state, after fusion, are the two means by which we may impress on metallic substances greater or less variations of density. I had made of pure silver, two plates well beaten with the hammer, and two cast, and very slowly cooled in their sand moulds: with them I formed a hollow rectangular prism, to which I added a metallic bottom: all these pieces were joined with soft solder, so as not to alter their densities or tempers during the operation. At the time of the union the four lateral faces were perfectly polished with pumice stone and coal dust, without the aid of the hammer or burnisher. We then took some coarse emery paper, and rubbed one of the cast and one of the forged plates with it in one direction very hard: the images of objects, which appeared very clear and intense on the faces to which we had left the beautiful polish, were com-

pletely effaced on those we had rubbed, which became dull and full of channels. The silver vessel thus prepared was filled with hot water. The four lateral faces successively turned against the opening of my thermo-electric apparatus, produced the following deviations on the galvanometer:

| | | | |
|---|---|---|---|
| 10° | for the forged and polished plate. | 13°,7 | for the cast and polished plate. |
| 18° | . . . . . . . . . . . . scratched . . . . | 11°,3 | . . . . . . . . . . . . scratched . . . . |

Comparing the four radiations, we perceive. *First.* That in the case of the polish, the cast metal gives nearly ~~one-third~~ more than the forged, which shows the influence of the less density. *Second.* That the effect of the channels on the two sorts of plates differs not only in intensity, as we have before seen it, but in construction; for if the radiating power of forged silver receives an augmentation of three-fourths by the action of the emery, that of the cast silver, on the contrary, experiences a *loss* of nearly one-fifth.

This unexpected fact, which proves in an undeniable manner the truth of our fundamental proposition, is perfectly explained in the theory we have just developed; for the pressure of a hard body, such as emery, on the soft surface of cast silver, slightly compresses and condenses the parts rubbed, and renders the bottom of the effected channels on one of the plates harder than the entire surface of the corresponding plate.

I regret not having been able to operate similarly on gold and platina vessels, where these manifestations would, in all probability, be reproduced on a more extensive scale, on account of the great variations of density which might be impressed on these two metals by fusion and percussion.

Returning now to the first observations of Leslie, we see that the different metallic plates, which he submitted to proof, constantly gave him a greater emissive power, when rough and irregular, than when smooth and polished. After that, nothing appeared more natural than to admit in the phenomena of calorific emission, besides the influence dependant on the quality of the superficial beds, a particular influence due to their degree of polish, at least for metals: this was also the conclusion drawn from the facts observed by Leslie, and yet, this conclusion so simple and direct in appearance, was not permitted.

This is an example which might serve, in need, to moderate the unfortunate facility with which certain experimenters hasten to make into general laws, the consequences resulting from their first observations. It often suffices to take an instrument in hand, and employ it in some research to fall on a new fact; but in pursuing the work with assiduity, varying the experi-

mental methods, and analyzing the phenomenon under different aspects, we almost always finish by perceiving that the novelty was only apparent, and the real explanation is found among the truths already classed in the science, or, if in the end there really comes a new truth from it, it is almost always contrary to those supposed general laws which were at first presented to our mind, in so clear and decisive a manner.

---

## XLIX. *Description of an Electrical Unit Jar. By* CHARLES GRIFFIN, ESQ., *in a letter to* WILLIAM STURGEON, ESQ.

Dear Sir,

Two or three years since I invented a piece of apparatus which I named an electrometer of absolute quantities, and inserted a description of it in the Mechanic's Magazine of the 3d of June, 1837.

When I came to town last July, I deposited a rough model I had by me, at the Adelaide Gallery of Practical Science, and Messrs. Bradley and Hook on testing its action were pleased to express a favourable opinion of its accuracy and probable usefulness.

Its value, as a meter, is founded on the fact that almost exactly the same quantity of electricity passes from the coating of a jar as is communicated to its lining, and vice versâ. I have since (by the merest accident) found that the same fact was, four or five years ago, made the foundation of a nearly similar instrument by Mr. Snow Harris,* the material difference being (I believe) that he applied his instrument to the measurement of very minute portions of electricity of low intensity while I was thinking only of considerable accumulations and high intensities which it will be evident required very different arrangements.

My remaining so long ignorant of Mr. Harris's invention, and Messrs. Bradley and Hook even not appearing to have been aware of its existence by their not mentioning its similarity to mine, furnishes very strong evidence of the absolute necessity of such a work as your Annals for the steady advancement of our beautiful and most useful science, and may, perhaps, be a sufficient excuse for my requesting the publication of a description of my instrument, with some observations

* A description of Mr. Harris's Unit Jar will appear in our next number. EDIT.

intended to make it more generally useful, particularly when applied to batteries highly charged.

A sketch, Plate XI, fig. 2, of the model (such as it is) deposited in the gallery may serve to illustrate.

A, is the meter jar. B, an eau de Cologne bottle, air tight, C D, a metallic sole to receive the bottoms of A and B, and fastened to E F. E F, a platform to keep all steady. *a a*, a metallic communication from the meter jar to the ball *b*. *b*, the knob of the jar. *c*, is the knob receiving the discharges of the meter and communicating by the brass tube *e* through the bottom of the bottle with the metallic sole and the coating of the jar. *d d* are two removable metallic forks on which is laid the charging jar, when one only is used, as the readiest mode of insulation. *f*, is a wire going to the earth to ensure perfect freedom of motion for the fluid.

A jar with two necks would, perhaps, be better and be more simple: one neck to communicate with the prime conductor and the other to receive the tube containing the discharging knobs.

In using this instrument the battery must be insulated and one coating placed in communication with the prime conductor and the other with the lining of the meter jar whose coating communicates with the earth. A certain portion of electricity being communicated from the machine to one coating of the battery, two portions, each equal to it, will pass, one from the other coating of the battery into the meter, and the other portion from the meter into the earth. Let the first mentioned of these two portions be just sufficient to cause the meter to discharge itself by the knobs and such portion will then be the unit of that meter. Another equal portion being communicated to the battery, another equal portion will charge and discharge the meter and form a second unit equal to the first, and so forth till the battery be fully charged. The number of discharges of the meter will (notwithstanding the differences of intensity in the battery at the different periods of charging) be a direct measure of the absolute amount of the electrical liquid in the battery.

The meter jar should discharge itself at a very low intensity, that the electricity intended to pass into it may not be dissipated, by passing slowly from one of its coatings to the other, or from the emittent side of the battery into the air. For the same reasons the coatings of the jar should be far apart and the exterior of the battery should be free from all points and roughness. The meter should, in consequence of its discharging itself at a low intensity, be large enough to give sparks at sufficiently long intervals of time to be easily

counted when a large machine is used. The discharging knobs should be of a metal least liable to be acted upon by sparks, and should be inclosed in a glass tube to exclude the dust and damp; and if the tube and jar also be air tight, it would perhaps be better, in order to prevent the discharge being affected by changes in the density and dampness of the air.

There is a source of error in my model in the expansion and contraction of the metallic tubes *a* and *e* of the meter by change of temperature, but which might be easily obviated by a different arrangement of the knobs. The communication of the battery, with the prime conductor and meter, should be quite continuous, the electricity never passing by sparks. The generation of the electricity should also be regular, to prevent irregular and extraordinary undulation of the liquid communicated to the recipient coating of the battery and causing correspondent undulations of that passing from the emittent coating through the meter. I think the communication from the outer coating of the battery to the meter, or from the outer coating of the meter to the earth, should, for a portion of the distance, be formed of a substance not capable of conveying a small shock, but, at the same time a moderately good conductor, as a moist thread or strip of linen for instance, in order, more effectually to resist, firstly, the undulations of the liquid in the battery and on the prime conductor; and secondly, the rush of the fraction of free electricity on the prime conductor into the battery, and the fraction on the exterior of the battery into the meter. The discharge of the meter would, very probably, enable this latter portion of electricity to rush through the meter in the path thus opened for it, and be an addition to each intended unit or absolute amount of *each* charge contained in the meter at the commencement, or first instant, of its discharge, which unit, or charge alone, ought to pass, and thus materially interfere with the accuracy of the instrument. The threads or other imperfect conductors should, if placed between the battery and meter, be (to prevent dissipation) inclosed in an Indian rubber or glass tube with metallic terminations.

Unless every part of the battery coatings be very far apart (or rather the exterior coating far from the top of the jar, for the distance between the top and interior coating makes no difference,) I doubt if this electrometer would do with very high intensities, as the electricity would otherwise pass over the exterior uncoated surface of the battery from the recipient to the emittent parts, and add to the absolute quantity of liquid passing through the meter; but this source of error

might, I think, be obviated by a ring of coating passing round the battery jars, a little above the outer coating, and communicating with a row of points, suspended by silk, at a distance from the battery, or with some not very imperfectly conducting substance in communication with the earth. If the rings communicated directly and perfectly with the earth they might occasion damage by a spontaneous discharge through the meter. At the same time the ring should not be too near the exterior coating lest it should take any electricity from the exterior coating which should pass through the meter.

I have thought that a balance with knobs might be usefully placed between the discharging knobs and made to move an index; but the beam must be a very delicate one; and the probabilities of inaccuracy from friction and other causes seem to more than counterbalance the trouble of counting the sparks. The only experiment I have tried is comparing the sparks that passed while charging a jar with those that did so while discharging it gradually by a point. When that jar was not highly charged, and every care used, the number of sparks in and out were equal, at least generally. When there appears one spark too few on discharging with the point, a considerable fraction of the unit or quantity necessary to produce that spark will be found in a free state on the coating of the battery connected with the meter, and, of course, in the meter itself. A means should be very carefully devised of forming

*A standard meter of unity;*

a standard that could be readily and exactly imitated at any time or place. This would perhaps be best effected by using a plate of air at a certain medium temperature and dryness, coated by two circular plates of metal of a certain diameter (one being insulated in the most simple and perfect manner) and discharging themselves by two spherical balls, also of a certain diameter. These balls and the plates being also at certain distances apart. Such a standard being once formed and preserved in a public institution, all other meters *otherwise constructed* might be regulated by it, by having their discharging balls set by a micrometer screw, to discharge with the same or any multiple or aliquot quantity of electricity.

To compare this standard electrometer with others, one coating (recipient) of each of two exactly similar plates of air might be made to communicate with a prime conductor; their other two coatings (emittent) to communicate one with the standard electrometer and the other with the one to be regulated by it. One of the balls of the latter should be moved till it be discharged at the same instant as the standard meter;

or (if it cannot be made to do so) till it is uncertain which will discharge first.* Between every trial the electricity to be of course completely discharged. The whole apparatus should also during the operation be placed at a distance from the machine and from every other substance capable of interfering by induction.†

I remain, dear sir,
Yours truly,
CHARLES GRIFFIN.

*Leamington,*
*Sept.* 24, 1838.

---

L. *Description of a galvanometer by which the deflecting power of an electric current, however copious, may be measured. Communicated in a letter dated April* 20, 1838; *Addressed to the Secretary of the London Electrical Society.*

Read May 5th, 1838.‡

Sir,

I beg to call the attention of the Society to a modification of the galvanometer, by which the deflecting power of any electric current, however copious, can be so reduced as to become measurable even by a delicate magnetic needle.

By this instrument, the electrical currents, above and below the needle, are made to move in the same direction; in which case, their deviating effects being opposed, no motion will take place in the needle as long as it is equidistant from both. But if the needle be not equidistant, then, naturally, the power of the nearest current will preponderate and cause immediate deviation: such deviation being the result of the difference between the two powers, and depending on the relative distances of the upper and lower wires from the needle. If then either of the reophores be made movable, the effective deviating power of the other is entirely under the control of the experi-

* A further method of testing the agreement of the new meter with the standard would be by frequently and moderately charging a battery by each, and discharging it by the other till their indications agreed.

† As every philosopher has a right to have his discoveries, or theoretical views, when published, bear the proper date of their being first made known, we beg to inform our readers that the substance of Mr. Griffin's two former papers which have appeared at p.p. 36, and 126 of the present volume, was made known to the Royal Society more than five years ago. Edit.

‡ From the Transactions of the London Electrical Society.

menter: but, for the sake of calculation, the distances from the needle, at which the movable current has one-third, one-half, two-thirds, &c., the power of the stationary one, must be graduated on the brass plate which has been added for that purpose. By the use of a winder and multiplying wire the graduation is facilitated, as the influence of the multiplier is felt through a greater space than that of a single wire. I have also added a double wire for the sake of allowing uninterrupted passages to any electric current, however copious.

The method of making one reophore movable by means of spiral wires, was taken from an instrument most obligingly shown to me by Professor Majocchi, of Milan, to which I have added the arrangement of the wires, by means of which the reaction of the currents and consequent reduction of their deflecting powers is accomplished; and if the London Electrical Society judges the instrument worthy of a distinctive appellation, I beg to propose that it be called the reacting galvanometer.

I remain, Sir,
Yours truly,
R. J. IREMONGER.

Bryanstone Street,
Portman Square,
April 20, 1838.

P. S. By using the winder of greater depth, an aperture may be made between the upper and lower coils so as to allow of the introduction of a needle; in which case the instrument becomes a common multiplier. Fig. 4, Plate XII., represents the instrument, where A is the winder carrying a coil capable of being raised or lowered by means of the sliding piece B, which moves tightly through the cross bar C C. D D, are columns supporting the cross bar and fixed at their bases to the sole G. These have the termination of the coil round them in spirals. E, the end of the spiral on column D, let into a groove in the sole. F F, mercury cups. The situation of the magnetic needle and graduated card is shown by the line and dotted circle; and the direction of the wire on the columns and winder, by the arrows. It will be perceived that since a current circulating through the wires moves in the same direction in the lower part of the coil above the needle, and in the terminating wire beneath the same, the coil will exercise a counteracting effect on the needle dependant on the space between them, and therefore that the deviations will be produced by the excess of power in the nearest or under current.

## LI. *British Association for the Promotion of Science. Eighth Meeting, at Newcastle upon Tyne: in August,* 1838.

(*Continued from page* 333.)

Mr. Whewell read a letter from the Astronomer Royal, G. B. Airy, Esq. on the means adopted for correcting the local Magnetic Action of the Compass in iron Steam Ships.'

"Royal Observatory, Greenwich, Aug. 21, 1838.

"Dear Whewell,

"Among the causes which have prevented me from attending the meeting of the British Association, the principal is, the trouble of carrying on a series of observations and experiments (at the request of the Admiralty) for correcting the local magnetic action on the compass, in the iron steam ship the *Rainbow*. Perhaps by communicating the principal results to the proper Section of the Association, you will more than compensate for my absence. The compass was placed in four different stations near the deck, and in four stations about 13 feet above the deck; and for each of these the ship was turned round, and the disturbance observed in many positions. The disturbances even at the upper stations were great, but at all the lower stations they were very great, and at the station next the stern they were enormous. The whole amount there was 100° (from —50° to +50°); and on one occasion, in turning the vessel about 24° the needle moved 74° in the opposite direction. I should have perhaps found some difficulty in reducing these to laws if I had not made some observations of the horizontal intensity at the four lower stations, in different positions of the ship. From these I was able to infer the separate amounts of disturbance due to the permanent magnetism of the ship, and to the induced magnetism, and to construct correctors. These correctors I tried yesterday, completely at the sternmost station, and imperfecty at two others. The correction at the sternmost station was (speaking generally) complete; the extreme of deviation which formerly exceeded 100°, did not, with the corrector, exceed 1°. At the other stations I had not leisure to adjust the apparatus; but I fully expect to-morrow to produce the same accordance at them. This result is, I should think, important, in a practical sense. Some theoretical results, which I did not anticipate, are also obtained. At the stern position, the disturbance is produced almost entirely by the permanent magnetism, the inductive magnetism producing

only $\frac{1}{15}$ of the whole effect. Going towards the head, the effect of the permanent magnetism diminishes, and that of the inductive magnetism increases, till the latter produces about $\frac{1}{3}$ of the whole effect. The resolved part of the permanent magnetism transverse to the ship, varies little (increasing somewhat towards the head): the part longitudinal to the ship decreases rapidly from the stern to the head (where it is less than the transverse part). I must not omit to mention that Mr. Baily took one department of the observations for one day, and will therefore be able to give you a complete account of the method of conducting the compass observations. In this, however, there is nothing very important: the principal object being to contrive methods of observing, in a place where no distant object could be seen, and where there seemed to be, at first, great reason for suspecting considerable local attraction peculiar to the place, and independent of the ship.

"G. B. AIRY."

Mr. Baily described the method of observing the deviation of the needle, caused by the immense mass of iron in this vessel, the *Rainbow*, by theodolites fixed in proper positions on the shore; the deviation of the needle, as the ship's head was veered round, was ascertained, when the needle on board was placed in different parts of the vessel.—Sir John Herchell said, that Barlow's compensating plate having been found inapplicable to the correction of the effect of such large masses of iron, it became a problem of much interest to find out an adequate correction, when the following principle was suggested by the Astronomer Royal: After the effect of the vessel upon the compass while on board had been determined, as described by Mr. Baily, the compass was removed to the shore, and placed in the neighbourhood of a large mass of iron, in such a way that the effect of this mass was the same as that of the vessel, a compensation for this was then applied to the compass; and upon removing the entire apparatus on board, it is obvious the ship, which is an exact equivalent for the mass of iron (now left on shore) must be exactly compensated also. A ludicrous circumstance had occurred, proving the necessity of this compensation. When they were bringing the vessel round from Glasgow, where she had been built, they had hazy weather, and at the Land's End they were under the necessity of hailing a vessel to know where they were. The crew of the other vessel were in amazement to conceive why a ship of such magnitude had been intrusted to such a set of land-lubbers.—Capt. Johnson, R.N. said, that Barlow's compensating plate was fully adequate to the compensa-

tion of such a mass of iron as that in the *Rainbow*, as he had frequent opportunity of proving; in fact, the maximum deviation of the needle would not be more than 13° when the compass was supended 18 feet from the deck.—Sir John Herchell begged not to be misunderstood: he had no intention to undervalue or disparage the compensating plate of Barlow, which was unquestionably a most valuable discovery.

*Major Sabine's address on the subject of Magnetic variations.*

I beg to occupy a few moments of the time of the section in reference to the report on the variations of the magnetic intensity, which the British Association have done me the honour to print in their last volume. I wish to communicate the results of the observations made by Capt. Duperrey in his voyage of circumnavigation in the *Coquille*, in 1822—1825, which I have only received in the present week in a private communication from that distinguished officer and magnetic observer. The section will learn with pleasure the satisfactory accord of these observations with those of Captains De Freycinet and Fitz Roy, published in my report. When in compliance with the wishes of the Association, I first entertained the purpose of collecting in one body the observations of intensity made by different observers in all parts of the globe, so far as they are comparable with each other, one of my first steps was to write to Capt. Duperrey to solicit the communication of any intensity results which he might have obtained. I find, by the letter which I have just received, that Capt. Duperrey did kindly comply with my request; but, unfortunately, the packet which must have contained the particulars of his observations has never reached me. The letter which I have just received contains a notice, both of the results he obtained, and of the mode in which they were observed. Had I possessed this information at the time my report was printed, I should on every account have rejoiced to have embodied it in the report; and I am anxious to avail myself of this opportunity of doing what may yet be done to supply the omission. Capt. Duperrey's observations were made with a horizontal needle, which, from accidental circumstances, was not observed with, prior to his departure from France. The usual test of the permanency of the magnetism of the needle, viz. its vibration at the same station at the commencement and at the close of the series, was, therefore omitted in this case. In the absence of this, which is the most conclusive test, Capt. Duperrey has estimated the loss which his needle may have sustained, by comparing its rate of vibration at

Paris on his return, with its rate at a station in Peru, in the line of no dip, in which comparison he has assumed the relation of the force at that station to the force at Paris to be as 1, to 1.3482. The loss of magnetism sustained by the needle on this estimation was altogether inconsiderable. The times of vibration at four other stations at which this needle was employed, corrected for temperature and arc, are as follows:—

| | | | |
|---|---|---|---|
| Payta ...... | 5° 6′S. .. | 278.50E. .. | 1.024 |
| Offak ...... | 0 2 .. | 130.44 .. | 1.079 |
| Port Jackson. | 33 52 .. | 151.12 .. | 1.617 |
| Isle of France. | 29 9 .. | 57.31 .. | 1.181 |

These determinations are inserted in a map engraved in 1832, referred to in a paper read by M. Duperrey to the Academy of Sciences, at Paris, in 1833, entitled, "Considérations sur le Magnetisme Terrestre." Capt. Duperrey notices, that at two other stations, viz.—Talcahuano and St. Catherine's, he observed the times of vibration of a dipping needle, the poles of which were reversed at each station, in the usual manner, for the observation of the dip; and that the results derived from the vibration of this needle, presuming it to have received on every occasion when the poles were changed, an equal magnetic charge, correspond in a remarkable manner—as indeed they do, with the subsequent observations of Captains King and Lutke; but Capt. Duperry, of course, attaches to these determinations no independent value, and therefore I need not notice them further. Capt. Duperrey has also communicated to me three results obtained at stations in France, in 1834, with one of M. Hasteen's needles, made, as it appears, with very great care, and with every necessary precaution. These results are, for

| | Lat. | Lon. W. Paris. | |
|---|---|---|---|
| Brest ............ | 48.24 .. | 6.50 .. | 1.365 |
| Landevence ...... | 48.18 .. | 6.35 .. | 1.363 |
| Orleans .......... | 47.54 .. | 0.26 .. | 1.341 |

Major Sabine next adverted to the observations of Prof. Bache and other gentlemen of the United States, which were not included in his report. These observations were made at New York, and in the adjoining states; and Mr. Bache is now engaged in connecting them with Europe, and, consequently, with the general body of the intensity observations. Until this comparison is complete, which it will not be until Mr. Bache returns to the United States, the observations referred to seem to determine the value of the magnetic force at the stations at which they are made, *relatively to each other*, but not *relatively to other parts of the globe.* It was for

this reason that they were not available for Major Sabine's report, which had for its object the general distribution of the magnetic force over the earth's surface. The American observations were made with magnetic needles inclosed in a vacuum apparatus, which Mr. Bache had devised, with the view of avoiding some of the anomalies occasionally experienced by other observers. They were made with extreme care, and were remarkable for minute attention to all those circumstances which conduce to the accuracy of the results.

---

## LII. *London Electrical Society.*

*Tuesday, October 2d.* Mr. Clark read a paper, entitled "Further Experiments in Magnetic Electricity."

In a previous communication* he had pointed out the advantages of thin wires over broad plates, for the terminal metals in the electro-gasometer, in the process of decomposing water by these machines. He had, since that time, made other experiments with two electro-gasometers in the circuit at the same time, one of which was furnished with *thin wire* terminals and the other with *broad plate* ones: the revolving coils being, in some of the experiments, composed of *long thin* wire, and in others of comparatively short thick wire; or, as the author terms them, the intensity and quantity arrangement. These experiments were compared with others in some of which one gasometer alone was in the circuit, and in others when the other gasometer was in the circuit. The following table was shown as representative of the results.

Time in liberating one cubic inch of the mixed gases.

| Long thin wire coils. | | Short thick wire coils. | |
|---|---|---|---|
| Gasometer with wires. | Gasometer with plates. | Gasometer with wires. | Gasometer with plates. |
| 16′ 40″ | 6h. 40′ unconnected. | 5′ 33″ | 1′ 42″ unconnected. |
| 14′ 26″ | 3h. 20′ connected. | 14′ 26″ | 6h. 40′ connected. |

Mr. Pollock also read a short paper, in which he endeavoured to support the theory of electric vibration by comparing the physiological effects produced by magnetic and common electricity.

*Tuesday, October 16th.* Mr. Walker read an account of a series of experiments which were made with a powerful

* See p. 336 of the present volume.

constant voltaic battery, at the house of J. P. Gassiot, Esq. Honorary Treasurer, Clapham Common; conjointly by Mr. Gassiot, Mr. Sturgeon, Mr. Mason, and himself. The battery consisted of 160 porcelain jars, each of which was furnished with a copper and zinc cylinder, with proper connecting pieces so as to be arranged either in series, or in any number of groups as might seem necessary for the purpose of varying the experiments. The whole battery was furnished by Mr. Gassiot. The zinc cylinder of each cell was covered with brown paper and afterwards placed in the copper. The liquids employed were salt water inside the paper envelope of the zinc, and solution of sulphate of copper on the outer side: consequently the zinc cylinder was exposed to the former and the copper cylinder to the latter liquid. The whole battery was divided into eight groups of cells, each group of twenty cells forming a distinct series, which could be employed either separately, or in combination with the rest, at pleasure. The decomposition of water, acidulated with sulphuric acid, was first tried with the individual groups: and afterwards by combining them in series forming batteries of 20 pairs; 40 pairs; 60 pairs; 80 pairs; 100 pairs; 120 pairs; 140 pairs; and 160 pairs. The following results were obtained: the *time* occupied in liberating one cubic inch of the mixed gases was taken as the standard of power of each individual battery.

| | | |
|---|---|---|
| *With 20 cells the time was | | 42″ |
| 40 | - - - - - - | 35″ |
| 60 | - - - - - - | 33″ |
| 80 | - - - - - - | 30″ |
| 100 | - - - - - - | 28″ |
| 120 | - - - - - - | 27″ |
| 140 | - - - - - - | 28″ |
| 160 | - - - - - - | 28″ |

* The results exhibited in this table, although in opposition to the generally received notions of electro-decompositions by voltaic batteries, are strictly in accordance with the views we have long entertained, and promulgated in our lectures on this subject. For, if any compound requires an electric force of a certain intensity only, to accomplish the separation of its constituents, it would be needless to augment the *intensity:* but by augmenting the *quantity* of electricity, at the required *intensity*, we multiply the *rate* of decomposition accordingly. And as different compounds require different intensities of electric force to accomplish their decomposition, so will they require different extent of series to produce that effect; and the *extent* or rate of decomposition will then depend upon the extent of the surfaces employed in the individual pairs.

Between every two decomposing experiments the battery current was directed through a galvanometer, made of a square coil, whose side was six inches long: but nothing very particular was observed. Mercury, zinc turnings, gold and silver leaf, &c., were deflagrated in a very splendid manner. But the most interesting calorific result was the following. The connecting wires of the battery were of copper and about one-tenth of an inch diameter. When the outer extremities of these wires were brought into contact with each other, whilst the whole battery was in series, they would adhere as if soldered together; which must have been the effect of a partial fusion of the copper. If now the wires were gently withdrawn from each other an electrical flame would play between them; and the extremity of that wire which was connected with the positive pole of the battery would become red hot in about a quarter of a minute: but the extremity of the *negative pole wire* which was opposite, and which *received* the fluid, never became heated to that extent. If now, the wires were placed across one another, at an inch or more from their extremities, the same kind of adhesion would take place as when placed end to end; and the intervening flame also played in the same manner when the wires were slowly separated to the distance of about half an inch. When the flame had played about half a minute between the wires the *positive* one, from the spot where the electric fluid left it, to its extreme point, exhibited a bright red heat: but the negative wire never became visibly red. Here we had two inches of copper wire, one-tenth of an inch diameter, which, though completely *out* of the electric channel became heated to the extent we have mentioned: but in no case did the negative wire become visibly red, even in dense darkness, though in some experiments the action was kept up for several minutes.†

Therefore, to produce maximum effects, it is necessary to ascertain the requisite intensity, and to employ no more: for, beyond that, the resistance in the extra cells of the battery more than counterbalances the augmented intensity. Precisely the same law is observed with magnetic electric machines. EDIT.

† As some of the particulars here described are not in Mr. Walker's report, it is very possible that we may not have mentioned them to that gentleman at the time we made this experiment; but as the fusion of the wires, and the heating of the positive one, whilst placed with its extreme point opposite to that of the negative wire, are as interesting as any of the results obtained, they cannot be too soon recorded. We have also thought it necessary to mention that the wires are first to be brought into contact, and afterwards

*November 6th.* Mr Walker read an account of some further experiments made at Clapham with the battery described in his former communication. In this, the *second* day's work, the decomposing power of the battery was again tried in nearly the same manner as in the former experiments; and although the results were not precisely the same as before, they present the same general character, as may be understood by the following table of them: the liberation of one cubic inch of the mixed gases being the standard as before.

| | | |
|---|---|---|
| With | 20 cells in series, the time occupied was | 37″ |
| | 40 - - - - - - - | 35″ |
| | 60 - - - - - - - | 35″ |
| | 80 - - - - - - - | 33″ |
| | 100 - - - - - - - | 33″ |
| | 120 - - - - - - - | 33″ |
| | 140 - - - - - - - | 32″ |
| | 160 - - - - - - - | 31″ |

When the power of the battery was tried on the decomposition of pure distilled water alone, the liberation of the gases was very slow, but increased with the extent of the series. The standard quantity of gas was 1-20 of a cubic inch; which was liberated by 40 cells in 5′ 30″, by 60 in 3′ 30″. by 80 in 2′ 35″, by 100 in 1′ 35″, by 120 in 1′ 25″, by 140 in 0′ 50″, by 160 in 0′ 40″. 20 cells produced an effect too insignificant to notice.

At the suggestion of Mr. Mason, the whole battery was arranged as a series of 20 pairs, by uniting all the *positive* poles of the 8 groups with a mass of mercury in one cup, and all their *negative* poles in a similar manner in another cup. When these compound poles were properly connected with the metals of the electro-gasometer, containing acidulated water, the rate of decomposition was one cubic inch of

gently separated; for we have never yet seen a spark, or flame, pass between the two prior to their being in contact.

We have long entertained the idea that the electric, magnetic, and calorific elements are perfectly distinct from one another: being unable to understand the production of their respective phenomena by any other means. The calorific matter we consider to be exceedingly inactive when compared with the activity of the electric matter; and as both require room or space to move in, a copious electric current traversing a wire displaces the sluggish calorific matter, and compresses it so as to produce heat, as decidedly as by mechanical means. The experiment before us, as well as the production of cold, as shown by Peltier, Dove, and Lenz, are all specimens of this kind of action. Edit.

mixed gases in every 7 seconds of time, as long as the battery was kept in action on the acidulated water. By this arrangement of the battery, the decomposing power is about $4\frac{1}{2}$ times that of the battery when in one continued series, when the compound operated on was acidulated water: a result which will serve as a guide in the future arrangements of voltaic batteries for similar purposes.

Whilst the whole battery was in series, a lump of native sulphuret of antimony was subjected to its action. The sulphuret being placed on the table, the ends of one of the battery wires was held firmly upon it; the other wire was now brought to touch the sulphuret at about an inch distant from the former; but as no effect was produced at that distance it was brought gradually closer to the other wire until the current passed. When a small portion of the sulphuret had become fused, one wire was gradually drawn farther and farther from the other, until a channel of the fused sulphuret (which in this condition was found to be a good conductor), of 3 inches in length, was obtained. The effect was really beautiful. The fused matter was thrown out of its bed and preserved. It was at first thought to be pure antimony, but on farther examination it was found to be a bright black sub-sulphuret, very compact, and exposing, when broken, a beautiful crystalline structure. For a complete account of these experiments we must refer the reader to the Transactions of the Society.

*November 20th.* A paper was read by Martyn Roberts, Esq. "On the use of Galvanism in blasting rocks." In this interesting paper several experiments were detailed which had been attended with perfect success, made on granite rocks in the neighbourhood of Penzance, Cornwall. Mr. Roberts's method of blasting is thus described:—In a hole of 24 inches, bored in the rock, is placed 3 inches of gunpowder: a wad of oakum is then driven in 9 inches from the top of the hole, leaving 12 inches of air between it and the powder. The upper part of the hole is filled with sand; a convenience being allowed for placing a thin iron wire in connexion with the powder, which wire becomes red hot, and consequently explodes the charge, when connected with the voltaic battery employed.

A paper from Mr. Sturgeon was also read, being an account of a repetition of one of Mr. R. Were Fox's experiments, on the lamination of clay by voltaic electricity. The apparatus employed were placed on the table; and the clay operated on was broken into several pieces by the assistant secretary, and examined with great interest by many persons who were

present; but no appearances of lamination could be discovered in any of the fractures: hence it was concluded that the experiment had failed to produce the anticipated effect.*

*December 4th.* A paper was read by Mr. Sturgeon, detailing a series of experiments on the action of caloric on the poles of a magnet, showing that magnetic poles may be driven from the point of heat in any required direction.

* Being unable, through indisposition, to attend the meeting, I sent the clay, still undisturbed in the pot in which it was placed during the whole time the experiment had been going on, to the assistant Secretary, requesting him to break up the clay at the meeting, which was done. I have since that time examined the clay myself; and cannot perceive the slightest tendency to lamination.

Understanding, however, that some singular remarks had been elicited in the discussion which took place on the subject of this experiment, and finding also that various accounts of it have appeared in public prints; I consider that it is due to Mr. Fox, who so kindly, and without the least reserve, furnished me with the necessary materials, that a fair description of the whole process of the experiment should be given by myself. With regard to the preparations and fitting up of this experiment, I cannot give any additional information to that given in the Annals for June of the present year. Vol. II. p. 475. The experiment commenced on the first of May, and the apparatus was not disturbed till the first of October; with the exception of the metals being twice taken out of the pot for examination, and the liquids replenished when previous evaporation rendered it necessary. The metals were never kept out of the arrangement two minutes at a time, certainly not five minutes during the whole five months: and being both times carefully replaced, this circumstance could have no possible influence on the general effects of the experiment. The zinc was a piece taken from a thin sheet, and was not much injured by corrosion at the conclusion of the process: hence, it is obvious that not much sulphuric acid had passed through the clay wall from the solution of sulphate of zinc, which was in the other cell: and, consequently, but very little voltaic action could have been carried on during the greater part of the time. This latter inference is partly corroborated by another fact which was observed on examining the metals when first taken out of the pot, which was about five weeks after the commencement of the experiment. The copper ore, at that time, was covered with a dirty leaden coloured matter which appeared to me to be an oxide of zinc, probably derived from a decomposition of a portion of the sulphate in which it was placed. That portion of the copper connecting wire which passed round the ore, was also covered with the same kind of matter: and both ore and wire retained their coating when finally taken away from the pot.

The clay wall was about an inch and a half thick, and when first formed reached to the top edge of the pot: but at the end of the

*December* 18. The reading of Mr. Sturgeon's paper was resumed and concluded. The Rev. Mr. Shillibeer's sustaining battery, consisting of 12 cells, and which had previously been presented to the society by that gentleman, was placed on the table for the first time. Its neatness and portability created much interest amongst those who inspected it.

Mr. Sturgeon made a few experiments showing the manner of protecting the power of steel magnets, from the action of converse electric currents, by placing them under the influence of other permanent steel magnets. Also other experiments showing that soft iron, when in contact with the poles of a magnet, facilitates the subduction of its power when assailed by a converse electro-magnetic force.

In consequence of the approaching holydays, it was announced from the chair that the next meeting would not take place till the 15th. January, 1839.

## LIII. MISCELLANEOUS ARTICLES.

### *To William Sturgeon, Esq.*

Dear Sir,

I shall not apologize to you for sending the following description of a voltameter which I have constructed for my own

five months it had settled down about three quarters of an inch, having its base spread outwards on both sides. When the metallic part of the arrangement was removed, there was still a little liquid left in each cell, which was also taken out: and the clay left to dry gradually.

Since the result of my experiment has been reported by the Electrical Society, I have, through the kindness of Mr. Jordan, been made acquainted with some further particulars essential to the success of the experiment, which, from their not being attended to, I may *possibly* have failed in mine. I have mentioned Mr. Jordan's suggestions to the Committee of the Electrical Society; and I am requested to make another trial as soon as possible. I shall employ six or eight voltaic pairs in series in my second trial, as suggested by Mr. Jordan; and I shall endeavour to prevent the wet clay from sinking in the pot, by supporting both sides of it with the polar plates, which, by this arrangement, may be of any required size. I understand that Mr. Jordan has succeeded in laminating pipe-clay by a voltaic action of three weeks' duration only: the operating battery being a series of four pairs. The particulars of my *second repetition* of this experiment will be made known in the next number of the Annals: and also the result of my repetition of Mr. Fox's experiment on the conversion of copper ore by voltaic action. See Annals, Vol. I. p. 133. Vol. II. p. 395, 476.

WILLIAM STURGEON.

use. Simplicity and cheapness are its recommendations, and I know you will consider these desiderata quite sufficient for my requesting its insertion in your excellent Annals.

*Description.*—Fig. 3, Plate XI. A, a confectioner's broad mouthed 16 oz. bottle. B, the bung fitted tightly into the neck of the bottle. C, a glass tube 18 inches long hermetically sealed at the top, and passed through the bung at *d*. The tube is graduated upon a slip of white paper covered with a varnish of lac. *e e*, two mercury cups for communicating with the battery, made with corks, and firmly fixed with sealing wax cement to the bung, through these cups and the bung two copper wires one-eighth of an inch in diameter, *f f*, are passed downwards, and at about 4 inches within the jar are bent at right angles, passing horizontally until they come beneath the opening of the glass tube, when they are again bent at right angles and passed upwards within the tube, and are terminated with platinum foil soldered to the ends. The platinum electrodes are one-eighth of an inch wide, and five-eighths of an inch long. The whole of the copper wire conductors within the jar and about one-eighth of an inch of the platinum, is well covered with sealing wax varnish, and the extremities are kept steady by being passed through a small piece of cork *g* just below the mouth of the tube.

It is scarcely necessary to state that the jar should be filled with the liquid to about an inch above the opening of the tube, and the tube is filled in the same manner as in the more expensive instrument, viz. by placing the fingers over the mercury cups, and turning it upside down.

I remain,

Dear Sir,

Your most obedient servant.

Islington,
Nov. 9, 1838.

G. MACKRELL.

---

*To the Editor of the Annals of Electricity, &c.*

31, *Lisle St., Leicester Square,*
*December* 3, 1838.

Sir,

Believing that any improvement in the apparatus connected with the science of electricity will always find a ready insertion in the Annals, I take the liberty of enclosing the following description of a modification (and I am induced to think a novel improvement) of the rotatory electro-magnet. The novel feature in the arrangement being the simultaneous rotation in an opposite direction of the permanent magnet with

the temporary, or electro-magnet. Fig. 1, Plate XII, will, perhaps, convey a better idea than any verbal description can. The current, from the voltaic battery, passing through the wire *a a*, instead of going directly to the mercury flood in connexion with the electro-magnet, is made to enter two concentric troughs containing mercury placed immediately under the former, the upper and lower cups being communicable by two wires, as seen in the figure. We are thus enabled to place the electro and permanent magnets on one spindle; the former being put in motion, the permanent magnet did immediately commence rotating in the opposite direction.

By the insertion of the above, you will greatly oblige,

Sir,

Your most obedient Servant,

C. W. COLLINS,

*Electrical and Philosophical Instrument Maker.*

---

*Description of an Air Pump of a very simple construction, which acts both as an exhauster and condenser.* By JOHN JOHNSTONE, A. M., *Professor of Natural Science in the Wesleyan University, Middletown, U. S.**

The last number of this (Silliman's) journal contains a description of a very ingenious air pump invented by Dr. Hare, professor of chemistry in the University of Pennsylvania, which is capable of performing on a much larger scale precisely the same operations as the one I am about to describe, but in quite a different manner. The next day after I had contracted with Messrs. Brown and Francis, in New York, for this air pump, which is now in possession of the Wesleyan University, I had the pleasure of viewing Dr. Hare's in his laboratory in Philadelphia.

This pump, as will be seen by fig. 5. plate XII, has two barrels, in which the pistons are worked precisely as in those in common use, and, in general, it is constructed in a similar manner. The pistons, however, are solid, and at the base of each barrel are two valves, one opening upward and the other downward. In the centre of the firm piece of mahogany, which forms the base of the instrument, are two brass tubes, which are seen in the figure at *a* and *b*, by the removal of the plate of brass D. Of these tubes, *a*, commences with the valves, one in each barrel, that opens *upward*, and *b*, with the valves that open *downward*. Now when either of the pistons descends, the air in the barrel below it will, of

* From Silliman's American Journal.

course, pass out through the downward opening valve and tube *b*, connected with it; and when it is again raised, the air will pass in through the tube *a*, and the upward opening valve. At the centre of the disc F, is an aperture, as in common air pumps, into which a tube may be screwed, and directly beneath it is another aperture communicating with the tube G: and the part *e*, *c*, is constructed in such a manner, that when *e* is upward, a passage is opened between the aperture F and the tube *a*, and also between the tubes *b*, and G. If the pump be now worked, it is evident the air will pass in at F, and out at G; that is, it exhausts at F and condenses at G. If, however, we give *e c* a quarter of a revolution, and bring *c* upward, the passages from *a* to F, and from *b* to G, are closed, and others opened from *b* to F, and from *a* to G: and by working the pump the air will now be made to pass in at G, and out at F, or in the reverse direction to that just described. This pump, therefore, like the one described by Dr. Hare, when worked is constantly exhausting and condensing. The uses to which this air pump may be applied, obviously include all those of a common air pump and condenser; and also enables the operator to transfer any gas that will not corrode the metals, from one vessel to another (as does that of Dr. Hare). To do this, it is only necessary to attach tubes at F and G, leading to the different gasometers or other vessels between which the transfer is to be made: and by means of the part *e*, *c*, the gas can be made to pass in either direction at pleasure. I ought to remark before closing, that previous to my application to Messrs. Brown and Francis, they had made several pumps of this description, with the exception of the tube G, which was added at my suggestion: and which adapts it in a peculiar manner for use in a chemical laboratory.

Fig. 1

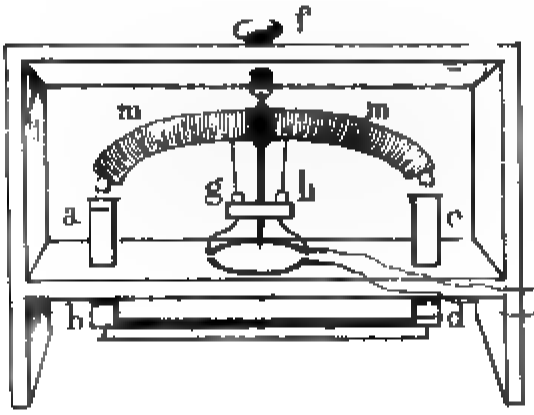



Fig 5.

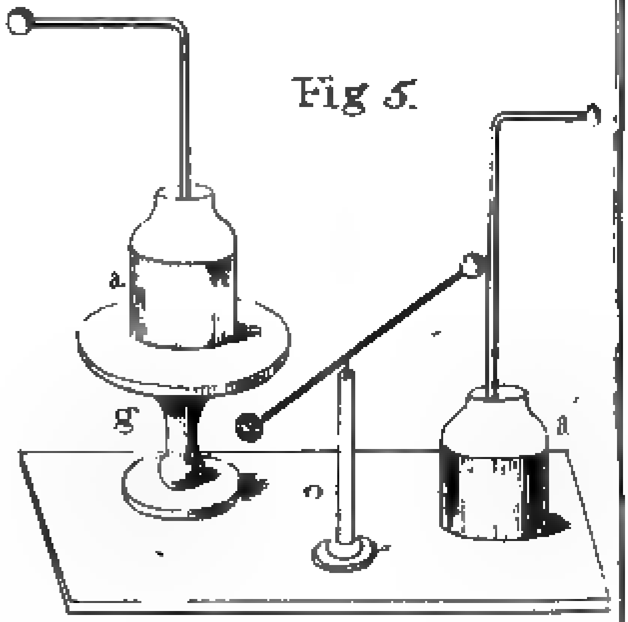



Fig 6

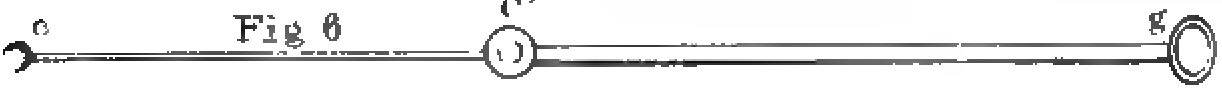



Fig 3

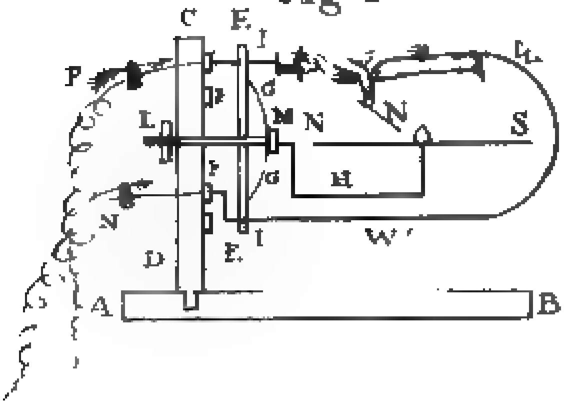



Fig. 4

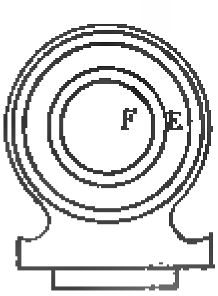



Fig 7

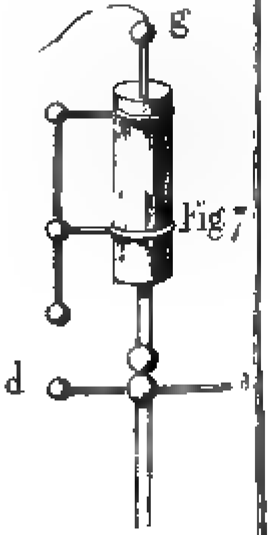
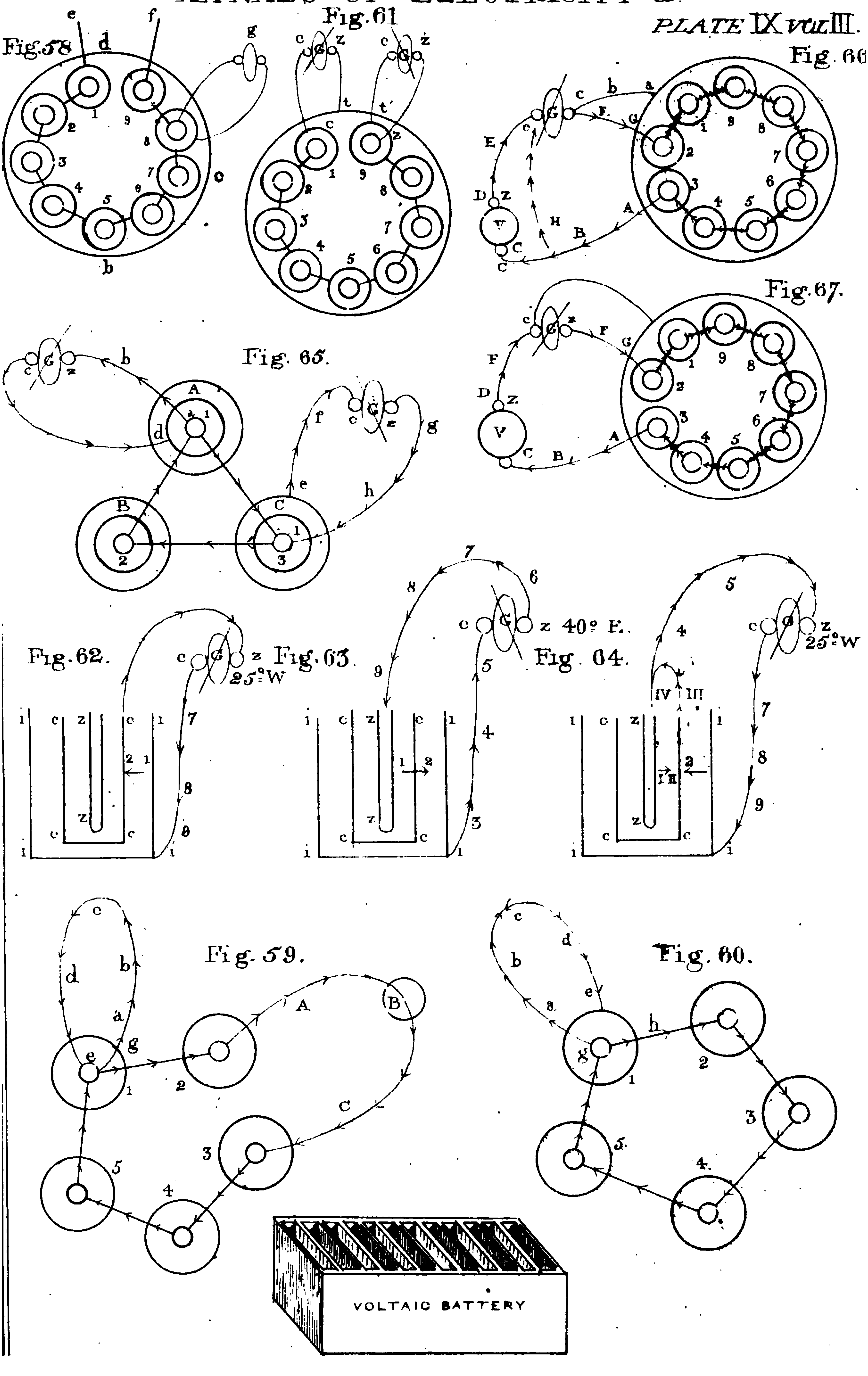


*PLATE IX VOL. III.*



# THE ANNALS OF ELECTRICITY, MAGNETISM, AND CHEMISTRY;

AND

## Guardian of Experimental Science.

MARCH, 

LIV. [illegible]

[illegible]

* Phil. [illegible] Vol. XI, p. 79. [illegible]
† Qua[illegible]
‡ Bak[illegible] p. 249.
§ Trans[illegible] Vol. XI.
‖ Transactions of the Society of Arts [illegible]

THE ANNALS

OF

*ELECTRICITY, MAGNETISM,*

*AND CHEMISTRY;*

AND

**Guardian of Experimental Science.**

*MARCH*, 1839.

## LIV. *Historical Sketch of the rise and progress of Electro-magnetic Engines for propelling machinery.*

To trace the invention of electro-magnetic engines to the first ideas which were formed in the minds of philosophers, concerning the probability of their structure and usefulness, would be an undertaking which no one could possiby accomplish; but, as has been the case in many other inventions, it may probably have been at a much earlier period, in the history of electro-magnetism, than the date of any contrivance, for that purpose, which has hitherto been described. The electro-magnetic rotations, first suggested by Dr. Wollaston,* and actually performed by the ingenious contrivances of Mr. Faraday,† were depending upon forces too feeble to anticipate from them an accumulation of power to a sufficient extent to be applicable in propelling machinery; though, as we are well aware, that subsequent forms of the apparatus, especially the stellar wheel of Mr. Barlow,‡ and the rotating disc of Mr. Sturgeon,§ on an extensive scale, were subjected to trials for this purpose; but, although voltaic apparatus and horse-shoe magnets of great powers, were employed in these trials, the combined forces were found to be insufficient to keep even the wheels themselves in motion. Mr. Sturgeon's discovery of magnetizing bars of soft iron to a considerable power, and rapidly changing their polarity by miniature voltaic batteries,‖ and the subsequent improved plan, by professor

* Phil. Trans. for 1823. Quarterly Journal of Science, Vol. XII. p. 79. Vol. XV. p. 289.

† Quarterly Journal of Science, Vol. XII. p. 74.

‡ Barlow's Magnetic Attractions, 2d. Edit. Part III. p. 280.

§ Transactions of the Society of Arts. London. Vol. XLIII.

‖ Transactions of the Society of Arts. Vol. XLIII. 1825.

Henry, of raising the magnetic action of soft iron,* developed new and inexhaustible sources of force which appeared easily and extensively available as a mechanical agent; and it is to the ingenious American philosopher, above named, that we are indebted for the first form of a working model of an engine upon the principle of reciprocating polarity of soft iron by electro-dynamical agency. Dr. Henry's machine is described in the twentieth volume of Silliman's American Journal of Science, in a letter to the editor of that excellent periodical. The following is a copy of Dr. Henry's letter.—

"*On a reciprocating motion produced by magnetic attraction and repulsion: by Professor Joseph Henry.*

*To the Editor.*

"Sir,

"I have lately succeeded in producing motion in a little machine by a power, which, I believe, has never before been applied in mechanics—by magnetic attraction and repulsion.

"Not much importance, however, is attached to the invention, since the article, in its present state, can only be considered as a philosophical toy; although, in the progress of discovery and invention, it is not impossible that the same principle, or some modification of it on a more extended scale, may hereafter be applied to some useful purpose. But without reference to its practical utility, and only viewed as a new effect produced by one of the most mysterious agents of nature, you will not, perhaps, think the following account of it unworthy of a place in the Journal of Science.

"It is well known that an attractive or repulsive force is excited between two magnets, according as poles of different names, or poles of the same names, are presented to each other.

"In order to understand how this principle can be applied to produce a reciprocating motion, let us suppose a bar magnet to be supported horizontally on an axis passing through the centre of gravity, in precisely the same manner as a dipping needle is poised; and suppose two other magnets to be placed perpendicularly, one under each pole of the horizontal magnet, and a little below it, with their north poles uppermost; then it is evident that the south pole of the horizontal magnet will be attracted by the north pole of one of the perpendicular magnets, and its north pole repelled by the north pole of the other; in this state it will be at rest, but if, by any means,

* Silliman's American Journal of Science, Vol xix, P. 329.

we reverse the polarity of the horizontal magnet, its position will be changed, and the extremity which was before attracted will now be repelled; if the polarity be again reversed, the position will again be changed, and so on indefinitely: to produce, therefore, a continued vibration, it is only necessary to introduce, into this arrangement, some means by which the polarity of the horizontal magnet can be instantaneously changed, and that too by a cause which shall be put in operation by the motion of the magnet itself: how this can be effected, will not be difficult to conceive, when I mention, that, intead of a permanent steel magnet, in the moveable part of the apparatus, a soft iron galvanic apparatus is used.*

"The change of polarity is produced simply by soldering to the extremities of the wires which surround the galvanic magnet, two small galvanic batteries, in such a manner that the vibrations of the magnet itself may immerse these alternately into vessels of diluted acid; care being taken that the batteries are so attached that the current of galvanism from each shall pass round the magnet in an opposite direction.

"Instead of soldering the batteries to the ends of the wires, and thus causing them at each vibration to be lifted from the acid by the power of the machine; they may be permanently fixed to the vessels, and the galvanic communication formed by the amalgamated ends of the wires dipping into a jar of mercury.

"The whole will be more readily understood by a reference to fig. 4, Plate XI, where A B is the horizontal magnet, about seven inches long, and moveable on an axis at the centre; its two extremities when placed in a horizontal line, are about one inch from the north poles of the upright magnets C and D. G and F are two large tumblers containing dilute acid, in each of which is immersed a plate of zinc surrounded with copper. *l, m, s, t,* are four brass thimbles soldered to the zinc and copper of the batteries and filled with mercury.

"The galvanic magnet A, B, is wound with three strands of copper bell wire, each about 25 feet long; the similar ends of these are twisted together so as to form two stiff wires, which project beyond the extremity B, and dip into the thimbles *s, t*.

"To the wires *q, r,* two other wires are soldered so as to project in an opposite direction, and dip into the thimbles *l, m*. The wires of the galvanic magnet have thus, as it were, four

* For a method of constructing the galvanic method on an improved plan, see my paper in Vol, XIX. p. 329, of this (Silliman's) Journal.

A description of Prof. Henry's method of making soft iron magnets by voltaic electricity will soon appear in these Annals. Edit.

inspecting the figure, it will be seen
hich dips into the cup attached to the
G, corresponds to the extremity of
in F.
are in action, if the end B is depressed
cups *s*, *t*, A B instantly becomes a
g its north pole at B; this of course
pole D, while at the same time it is
tion is consequently changed, and a
n the mercury in *l*, *m*; as soon as the
ed, the poles are reversed, and the
If the tumblers be filled with strong
is at first very rapid and powerful,
rely ceases. By partially filling the
d, and occasionally adding a small
uniform motion, at the rate of 75
has been kept up for more than an
ry and very weak acid, the motion
an indefinite length of time.
escribed, is entirely distinct from that
magnetic combinations of wires and
ectly from the mechanical action of
lvanism being only introduced for the
poles.
Green, of Philadelphia, to whom I
hine in motion, recommended the
agnets for the two perpendicular steel
this kind was to be constructed on a
undoubtedly be the better plan, as
n be made of any required power.
us, intended merely to exhibit the
described is perhaps the most con-

This ingenious invention of Professor Henry does not appear to have been any further pursued, nor any improvement in the original instrument to have yet taken place, though it is obvious that for some purposes, where a reciprocating motion is wanted, as in the pistons of pumps &c., it might be brought into play with more facility than most other forms of electro-magnetic engines that have hitherto made their appearance.

In 1835 Dr. Schulthess published a small work in which he speaks at considerable length about Dal Negro's* apparatus, or rather about the unintelligible manner in which it is des-

* Dal Negro's was rather for experiments than for work.

cribed; and then gives a description o
had made and exhibited in a lecture w
Philosophical Society at Zurich in
Schulthess's miniature machine consi
soft iron electro magnets, placed horiz
opposite to each other, with a small
play of another small magnet which
motion. The whole was supported upo
frame resembling the frame which sup
pump. See Taylor's Scientific Memo

The first rotatory electro-magnetic
is any account on record, is that inve
as described in the first volume of
engine was constructed in 1832, and
of this class which gave motion to wor
dry or, indeed, to which any model o
This engine would draw light loads on
saw thin pieces of wood by means of
worked with considerable facility: an
were of small dimensions, yet when
amalgamated,† and enveloped in eithe
or paper case, to prevent the transmis
the copper surface, and also to preven
exciting liquid,‡ employed, the acti
some considerable time. But the fo
means, though it would last much l
being so energetic as when dilute
employed for the exciting liquid of th
able part of Mr. Sturgeon's engine w
which carries four steel magnets, who
sions operate in concert, and in unis
and repulsions of four vertical soft iro
are brought into play by the electric
batteries: and the reversal of their p
four wires which, in succession, come into contact with four
quadrantal copper plates.

* Annals of Electricity, &c. Vol. I. p. 75.

† The history of amalgamated zinc being employed in voltaic batteries, will be found in Vol. I. p. 81.

‡ Sir H. Davy appears to have been the first philosopher who employed two or more different exciting liquids at the same time in the voltaic battery. He was thus enabled to construct batteries which consisted of one metal only. Nicholson's Journal, quarto, Vol. V. pp. 78, 341. Phil. Trans. for 1801, and 1826.

Professor H. Jacobi "constructed a magnetic machine for continued circular motion," in May, 1834,* which was described in a note presented to the Academy of Sciences at Paris, in November of the same year.† Professor Jacobi published an excellent essay on electro-magnetism as a motive force, and on its application as a first mover in machinery. A translation of this essay, from the French original, by Mr. Lang, will be found in the first volume of these "Annals," commencing at page 408. Jacobi's engine consisted of sixteen soft iron magnets, eight of which were fixed to, and rotated with, a horizontal shaft or spindle; the other eight being fixed to a steady frame in such a manner that their poles might be approached as close as possible, without being touched by those of the moveable magnets in their rotation. The whole of these magnets were brought into play by four voltaic troughs, whose electric currents traverse spiral conducting wires which surround the iron bars. Besides a description, with an illustrative plate, of this engine, much curious and interesting information will be found in the author's essay.

At the meeting of the British Association at Dublin, in 1835, the Rev. J. W. Mc.Gauley produced an electro-magnetic engine which attracted considerable attention. The author proposed to augment its power by several improvements which he then had in contemplation. At the meeting at Bristol, the following year, Mr. Mc.Gauley again brought forward his engine, and detailed an extensive series of experiments, the results of which did not hold out to him much further prospects of making his apparatus applicable to general use.

Professor Callan, of Maynooth College, who has published some valuable papers on electro-magnetism, was constructing a large engine on this principle in June, 1836.‡ This engine was tried towards the latter end of the same month, but was found defective in its first form: but by a new arrangement it gave rapid rotation to a wheel which weighed about one hundred pounds.§ From the prospects afforded by this small engine, Professor Callan was induced to proceed to the structure of one of much larger dimensions; which was to be worked by forty electro-magnets, and expected to propel a

* Annals of Electricity, &c. Vol. I. p. 410.
† Ibid p. 409.
‡ Annals, Vol. I. p. 378.
§ Ibid, 494.

carriage and its load weighing thirteen hundred weight, at the rate of seven or eight miles an hour. The author has entered on some interesting calculations on the probable advantages of electro-magnetic engines, which the reader may consult at page 494, Vol I. of these Annals.

In November, 1836, Dr. C. Page, of Salem, United States, who is the author of several beautiful pieces of electro-magnetic apparatus, turned his attention to engines of this kind; and published an account of his progress, in the American Journal of Science, for October, 1837. The revolving iron electro-magnets in this philosopher's contrivance, are of the horse-shoe form; and arranged on arms, or radii, at right angles to the rotating shaft, like the spokes of a wheel; having the poles outwards and nearly in the circumference of a circle in which the stationary magnets are fixed. Dr. Page mentions a large engine which he was then constructing, whose revolving apparatus would weigh nearly a hundred pounds. See these Annals, Vol. II. p. 216.

In April, 1837, Mr. Sturgeon announced his having succeeded in propelling a boat, and also a locomotive carriage by electro-magnetism. Annals of Electricity, Vol. I. p. 250.

The electro-magnetic engines constructed by Mr. Davenport of Bandon, near Rutland, Vermont, United States, were first announced in the American Journal of Science, for April 1837. (See Annals of Electricity Vol. II. p. 257). Since that time various accounts of these engines have appeared through the same medium, and others by the medium of the Journal of the Franklin Institute; all of which have been transferred to the pages of these Annals. (See Vol. II. pp. 257, 284, 347, &c.) Mr. Davenport's efforts, in bringing electro-magnetic engines into repute, have been strongly supported by the liberality and patronage of Mr. Ransom Cooke, and the apparent success with which they were attended, has been the means of forming an electro-magnetic association in America.*

In February 1837, Mr. Davenport secured an American patent for "the application of electro-magnetism to the propelling of machinery" in his own name;† and in the Summer of 1838, a Mr. Coombes was sent to this country with a locomotive engine of Mr. Davenport's construction, for the purpose of securing a similar patent for these realms.‡ We are not aware why Mr. Coombes returned to America without accomplishing the object of his mission; but we imagine that

* Annals, Vol. II. p. 284.
† Ibid, p. 347.
‡ Annals, Vol. III. p. 156.

difficulty in discovering that Mr. Daven
cipated, in almost every particular, in his
occupy too much room in this place, the
of the various accounts which have been
erformances of Mr. Davenport's engines,
it be necessary to do so, since they are to
in the different parts of these Annals to
eady alluded.
n Journal of Science for October, 1837, Dr.
gives an account of an electro-magnetic
s upon the principle of the electro-magnetic
poles of a horse-shoe magnet. We refer
II. p. 123, of these Annals for the author's
own account of this instrument.

An exceedingly ingenious arrangement of electro-magnets of soft iron was invented by J. P. Joule, Esq. of Salford, Manchester, in 1837, and described in these Annals in the January No. for 1838. (See Vol. II. p. 122.) The bars, in this beautiful instrument, are of a peculiar construction, and the transposition of their polarity effected by an exceedingly ingenious contrivance. Mr. Joule proposes to apply his engine both to locomotive carriages and to boats. The description is accompanied by two neat figures of illustration.

other small engine which the vast
to consist of forty magnets
e with a series of interesting ex
tleman, will be found at the latter

cribes a very pretty arrangement
in a letter to the Editor of the
d June 12th 1838. (See Annals
key's plan not only differs from
but appears to be well adapted for
moveable magnets, forty-eight
h the usual wire coils for the trans
; and arranged in eight groups of
volving central shaft. The fixed
d and number, and also arranged
in eight groups around the circle described by the outer poles of the moveable ones, in such a manner that both systems of magnets may operate mutually on each other and produce rotatory motion. See fig. 9, Plate I., Vol. III.

About the latter part of the year 1837 a variety of ingenious contrivances, in the form of rotating engines, were to be seen in the different philosophical instrument makers' shops in London. Some of them had miniature saw mills, pumps,

and other pieces of machinery attached, and p
by them. Mr. Watkins gave a description o
Phil. Mag. for February, 1838, consisting of
steel magnets placed at equal distances from
the circumference of a circle; and four small
magnets which were fixed at right angles to, and
a vertical spindle, situated in the axis of the
Palmer, of Newgate Street, has long had in his
great variety of rotating engines, with attached
chinery which are kept in motion by them. It
bable that every individual instrument maker h
liarity in the mode of fitting up this class of app

We close this general historical sketch of
hitherto been accomplished in the application o
netism as a motive power, with the following
another ingenious contrivance by Mr. Joule.

---

*Description of an Electro-magnetic Engine, with experiments, in a letter to the Editor of the Annals of Electricity, &c.*

New Bailey Street, Salford, December 1, 1838.

Dear Sir,

In Vol. II. p. 122 of your interesti
cation of mine, describing a method o
netic engines, which I thought m
advantage. I hoped that when the e
structing was finished, I should be abl
account of its duty. The engine was
mer. It weighs 7½ lbs. The utmost p
ceeded in developing, on working it
eight four-inch plates was equal to
per minute.*

The very inferior power of the engine convinces me that the distances between its poles was much too little, and that the magnets should have been fewer in number, and of much greater bulk.

I was desirous, before I attempted to make another engine, to satisfy myself by experiments, conducted on a smaller scale, how far it was possible to increase the velocity of rotation. Now, of the many things which prevent an infinite velocity, the resistance which iron opposes to the instantaneous induc-

* In this estimate the enormous friction of the engine was accurately measured and reckoned as the load; the velocity of the moving magnets was about 8½ feet per second.

tion of magnetism is of considerable importance. I think I shall be able to show how this may be obviated in some measure.

The power of the electro-magnetic machine is much increased by the insertion of a bundle of wires instead of a solid nucleus of iron into its coil; this phenomenon is evidently occasioned by the peculiar texture of the wires, which allows Mr. Sturgeon's magnetic lines* to collapse with greater suddenness.

With a view to determine to what extent the velocity of rotation might be increased by the use of wire magnets, I constructed an apparatus as represented by fig. 1, Plate XIII., where *a b*, *c d*, are the steel magnets; *e*, *f*, brass screws with very minute holes in their ends to receive the fine points of the steel axle on which the electro-magnet *m m* is fixed; *g*, *h*, are mercury cups to connect the wires of the electro-magnet with the pieces of watch spring *i*, *k*, which dip in the mercury cells; *w*, *w*, are wires which connect these cells with the battery; the distance between the poles *a*, *c*, was eight inches. I had four electro-magnets, which, with their spindles &c., could with great expedition be put off or on the machine, by means of the screws *e*, *f*.

No. 1 electro-magnet was made of a round bar of iron 1090 grains weight; No. 2 of nineteen iron wires of about $\frac{1}{20}$ inch diameter, weighing 642 grains: no particular pains were taken in annealing the iron and wires of these magnets. No. 3 and No. 4 magnets were made of iron and wire of the same quality and dimensions, but were annealed with very great care by a process in use among the manufacturers. Each of the magnets was first enveloped with a double covering of muslin, and then wound in the same manner with eleven yards of copper wire of $\frac{1}{40}$ inch diameter, well covered with silk: care was taken to make the friction of the pivots equal in each.

Here are the results of some experiments which I made with this apparatus. The numbers given in the table are revolutions per minute.

| | No. 1. Hard iron. | No. 2. Hard wire. | No. 3. Soft iron. | No. 4. Soft wire. |
|---|---|---|---|---|
| With a single constant battery. | 146 | 177 | 196 | 192 |
| With two constant batteries arranged for intensity. | 233 | 274 | 283 | 321 |

* See Mr. Sturgeon's Theory of Magnetic Electricity, Vol. I. p. 251, 277.

| | | | | |
|---|---|---|---|---|
| Ditto with weaker charge | 196 | 173 | 224 | 209 |
| Mean. ............... | 192 | 208 | 234 | 241 |

These experiments were frequently repeated with similar results, and every precaution was taken to guard against error. I obtained much greater velocities than those given above, which I rejected partly because I could not count them, and partly because the resistance of the air began seriously to affect the results.

Some allowance should be given to the wire magnets; for while the weight of iron in them was little more than half that in the others, their bulk and, consequently, the resistance of the air remained the same.

The sparks and shocks, on breaking the battery circuit, were hardly sensible in No. 1; twice as great, at least, in No. 2 and 3; in No. 2 they were a little greater than in No. 3; but by far the most brilliant and powerful in No. 4.

I intend to make another engine soon, and shall construct its magnets of wires drawn from rectangular holes; if I have more success with it than I had with my last, you shall hear from me again.

I am, Dear Sir,

Yours truly,

J. P. JOULE.

---

## LV. *Upon Telegraphic communication, especially by means of Galvanism.* By Dr. Steinheil, *Professor of Mathematics and Natural Philosophy, at the University of Munich, &c. &c. &c.**

Telegraphic communication may in its most general sense be defined as the method employed by one individual to render himself intelligible to others, and being, when viewed in this light, synonymous with intercourse is no human discovery, but one of the most wonderful gifts of nature, not to man alone, but in common with him to all social creatures is granted the faculty of communicating his sensations to others, and of exciting in them conditions similar to his own. Communication is the most powerful tie of the living creation; it connects one individual existence with another, reproduces in one what is granted to all, thus forming out of individuals, species, which in their turn present themselves as organic beings.

* Translated from the German by Julian Guggsworth.

There is assuredly nothing in nature more calculated to call forth our admiration than the contemplation of the variety of the means employed for attaining this mutual intelligence. From the undeciphered hieroglyphic signs of insects up to the complicated speech of man we observe a series of the most varied possibilities of reciprocal intercourse. In man, however, this gift of nature has attained an astonishing development, a development which in the form of speech and writing keeps pace with the march of his improvement, a development to which, as in the case of that improvement itself no absolute limits are assigned, and which equally with that improvement will struggle against those fundamental shackles of matter, time and space, till it extends them, fixed though they be, up to the unchecked ranging of mind. And as writing lays shackles on the passing sound and redeems it from fleeting time, so in like manner are the remotest distances to be annihilated and thoughts to be interchanged in an instant with those afar. The means of accomplishing this do not however lie directly within our reach; but by the patient observance of the powers and the phenomena of nature we render these subservient to us and make them the bearers of our thoughts, and it is this task which in the ordinary acceptation of the word is termed telegraphic communication.

In the works that have appeared, connected with this subject, we see that the natural phenomena thus applicable to the transmission of thoughts to remote distances are by no means few in number. We likewise at the same time observe that the choice of the signals, representatives of the ideas we transmit, admits of much variety. It must, however, be confessed that none of the proposed plans appear to amount to more than simple discoveries, and seem never to have originated in a leading fundamental idea of what conditions it is essential and indispensable to have in view. It has hitherto been considered sufficient to add another new possible means of corresponding to those already known, without examining whether there are not yet other methods better suited to the purpose and of greater simplicity. The more advisable course would be to establish at the outset what conditions are indispensable in complete telegraphic communication, and not to grapple with any difficulties with which the practical application of the scheme may be beset, for in most cases the method of carrying a plan into effect suggests itself to us as soon as we form a clear idea of what we have in view. Let us now make the attempt.

What we propose to attain by telegraphic communication is the transmission of ideas with the utmost rapidity at all times and to any distance; and when the distances are but moderate this problem is at once completely solved by speech. Hence for more considerable distances nothing more is required than that the telegraph should imitate speech. It appears to be evidently not the shortest way to make the telegraph imitate an imperfect method of communication. The signs, for instance, made use of by the deaf and dumb; it should be made to copy the most perfect of all methods of communication,—speech, in which the sound as it falls on the ear of itself arrests the attention, and is at once understood.

At the first blush, this appears to be a very difficult task, inasmuch as oral communication has such different sounds at its disposal, and is hence enabled to convey notions by means of a few combinations; and this difficulty no former arrangement of the telegraph has been completely able to surmount. Inventors previous to Gauss were bent on producing a vast number of distinct signals, without reflecting that this was only to be attained at the cost of simplicity. It escaped them that a rapid communication is to be attained by other means besides a multiplicity of signs, and that in point of fact one single sign, if it can be repeated with sufficient rapidity and employed in groups properly combined, is all that we require. In order to explain this I should wish to enter upon the analysis of our written language.

I select the great Roman letters for this purpose. They consist of six different signs, namely, a straight line in four positions, horizontal, perpendicular, sloping from left to right, and from right to left, and lastly a semicircle turned to the right or left. Of these six different signs never more than four are met with in the same letter, as in M and W. Now if we calulate how many different letters may be formed out of these six signs, never using above four of them however in the same group, we find the number amounts to no fewer than 1554; whereas we require but 25. We see from this example how much more numerous than the case called for has been the amount of signs employed in our written language.

Let us now suppose that we have but two kinds of signs and see if these are not quite sufficient for a complete series of letters. Let the signs be as simple as possible, each a dot for instance, and let them be distinguished by their position, that is to say, by one being always on a higher line than the other. Now if it was an understood thing that in each sign there was not to be more than a single dot, it is clear there could be but two signs; if two dots might be united in the

same sign, four more new signs would have to be added to the two above, giving in all therefore six different signs. Assuming that three dots, but no more, might be combined for one sign we should get eight in addition to the above, making therefore in all fourteen distinct signs. Supposing, in fine, that four dots can be united for one sign would give us sixteen more signs, making in all therefore thirty, a number quite enough for giving not only all the letters but the numerals as well. Two signs may however be dispensed with, if instead of a second sign a quick repetition of a single one is employed. Hence we see that one sign, a dot for instance, enables us to render writing with greater conciseness than by the use of the various signs we ordinarily use; and what *one* dot is as the simplest sign for writing, *one* sound is for speech. By repetition and by combination we may by habit form a language intelligible to the ear.

We are now therefore enabled to state the conditions which a telegraph of the simplest possible construction must fulfil. It need communicate but a *single sign* but must give that with the *utmost rapidity*. If it was besides to be got up in the most convenient method this sign should be made *audible*.

Let us now enquire in succession to what natural phenomena we may have recourse in order to make this single sign as we have described.

Light is only partially applicable to this purpose. Its transmission may, it is true, be considered as infinitely rapid, but we shall never be able to make it affect more than our sight. Moreover owing to the straight path it takes and the spherical shape of the earth, there are certain limits beyond which its use is not applicable. A telegraph whose signals are founded on the use of light can therefore only communicate with other stations when the attention of the observers at those stations is drawn to it, when the distance does not exceed a moderate number of miles, and when the weather is favourable. Notwithstanding these very confined limits imposed on its use the telegraph as constructed by Chappe has met with a favourable reception, and since 1793, when the first telegraphic line was established in France, been very generally adopted. It is remarkable that all the improvements proposed to be made in it should be but of minor importance. But although the principal defects, common to optical telegraphs, cannot of course be got over, yet it strikes me to be capable of being considerably improved. I shall here confine myself to the following.

The first thing must always be making the communications as rapidly as possible, and there are two methods of reducing

the time required for this. It can be brought about by stenographic contractions, in other words, by the introduction of *numerous* signals, but the employment of *a few* of which any idea may be rendered. The same can be also attained by diminishing the time required for giving the signal, and this appears the preferable method. All the motions of long levers, of which our telegraphs are composed, and of which in order to be seen at a distance they must always consist, require considerable time, in accordance with the laws of dynamics. Hence it would be well to retain the present mechanical arrangement of the telegraph no longer. The proposal of Gauss, to which he was led by his experience in the transmission of signals by means of the Heliotrope, appears to me to be capable of being employed in practice without difficulty.*

It is well known that a glass mirror of not above a few square inches in size, appears to the naked eye like a star at the distance of five and twenty miles and upwards, when so directed as to show an observer part of the image of the sun. For the time there is no sunshine, during the night, or when the weather is cloudy for instance, recourse may be had to the glare produced by directing the flame of hydro-oxygen gas upon lime. The signals would consist of flashes produced by turning or covering the mirror. This arrangement, a more detailed account of which in this place would detain us too long, would present the advantage of giving the signs with great rapidity. It is easy to distinguish six separate flashes of light in a second, and which on their disappearance leave a corresponding impression on the memory, as is the case with tones in rapid succession. We could therefore give at least thirty signs by means of flashes, while one is now given by the telegraph. There would further be the additional advantage of dispensing with telescopes, and, what appears to be of still more importance, only the observers at the stations would notice anything of the signals. I do not however deny that these advantages are more than balanced by insurmountable defects.

If we wish to carry on telegraphic correspondence without the attention of those at the other stations being previously called, we are *compelled* to affect the sense of hearing, whose impressions are alone constant, and which, within certain limits, allows us liberty of motion and permits us to occupy

* For a detailed description of the construction and use of the Heliotrope of Gauss see "Breithaupts Magazin von den neuesten Mathematischen Instrumenten" 2tes. Heft. p. 47. Cassel 1835.

ourselves with other affairs. But in order to produce a sound that may be audible without difficulty, some piece of mechanism must be put in motion at the other station and made to strike on a bell or do something of that kind.

One would say it must be by no means an easy task to transfer a power to a great distance and cause it to perform there, certain functions at our pleasure, and yet there are several possible methods of effecting this. Sound, radiant heat, electric and galvanic currents, may, when applied in certain ways, be employed for this purpose. The practical application however of each of these methods presents its own particular difficulties, and each carries with it its own inseparable defects, so that it appears advisable to examine them more closely in order to pronounce which of them presents the greatest advantages in its application.

We first come to the ordinary transmission of sound through the air, to the voice strengthened as it were by means of speaking trumpets. But this manner of giving signals can only be employed with effect when the distances are but trifling. The boatswain's whistle, the signal horn, and the alarm bell, are all similar methods, answering their particular purposes but inapplicable to telegraphic communication at considerable distances, inasmuch as any considerable increase of a sound would be too annoying to the immediate neighbourhood, but principally because the transmission of sound through the air, at the rate of about 1142 feet in a second, is not rapid enough. The case is different in the transmission of sound through water. Here the rapidity is nearly four times as great, and forms at least no longer an insuperable obstacle. The experiments which Beaudent instituted at Marseilles and the more recent investigations of Colladon and Sturm at the Lake of Geneva* put it beyond a doubt that one can transmit audible tones through water to a distance of many miles. The nature of the transmission of the tone is however of such a kind that one not only hears it but is also enabled to render it visible as a molecular motion by means of a particular apparatus, the lever of contact. Consequently in place of the troublesome method of hearing under water a plan may be contrived which by the trembling motion of the particles of water shall repeat that motion on a larger scale, thus placing the mechanical power we require for producing a sound directly within our reach. In this way of carrying on correspondence, and which we merely notice here

* Ann. de Chimie et Phys. XXXV. 113. Gilbert's Annalen, LXXXVIII. 39.

in passing, it appears not unimportant when compared with telegraphs founded on optical principles, that the communications may be made under all circumstances. Consequently should it be established that sounds are transmitted through the plane surfaces of the water we have all around us, of which there is hardly a doubt, and if the employment of this method might therefore be generally applicable, the subject will merit a more complete investigation, the more so from its not requiring any particular conductor for the transmission of the sound, and from the circumstance of no interruption of the signals being to be anticipated.

"Another possible method of bringing about transient movements at great distances
conductor is furnished by r
of condensing mirrors upo
vanic current is called int
to produce declinations of
attending the constructio
tainly considerable are nc
a telegraph however wou
those based on optical p
require the constant atten
optical one it would cease
hence partakes of the intr
method therefore is not p
with others.

The three principles of telegr
forward, namely, light, radiant
sound, have this in common, th
should be a direct connexion be
air, water, and the ground, for t
conductors excited or rendered
making the communications.
they differ advantageously from
will now proceed to consider a
with which frictional electricity
conductors called forth as long
idea of employing it for telegrap
Monnier in the year 1746 di
through a wire of considerable
Tuileries Fleies formed a part o
Paris produced shocks throu

* Priestley's History of Electricity, p. 59.

† Phil. Trans. Vol. XLIV. p. 290.

to 12789 feet. Watson* extended the experiment over a space of four miles near Shooter's Hill, composing his circuit of two miles of wire and an equal distance of dry ground. Lomond† transmitted telegraphic signals to a neighbouring room by means of a pith ball electrometer acted upon by frictional electricity. Reiser‡ illuminated, by the electric spark, letters, formed upon plates of glass with strips of tin-foil. Gauss§ makes mention of a communication from Humboldt according to which Bétancourt in 1798 established a communication between Madrid and Aranjuez, a distance of 26 miles by means of a wire through which a Leyden jar used to be discharged which was intended to be used as a telegraphic signal.

All these experiments put it beyond a doubt that frictional electricity may be employed for giving signals at any distances; and that when these signals are properly contrived they offer convenient means of telegraphic intercourse.

Frictional electricity has besides, as Gauss has already observed, the great advantage of not losing any of its force by increasing the length of the conducting wire, inasmuch as the whole of the electricity of one coating of the jar must traverse the entire length of the wire, be it what it may, to neutralize that of the other coating.

The experiments which Wheatstone instituted with a mirror in rapid motion to ascertain the rate at which the electric spark is transmitted through copper conductors, show that it travels with a velocity of 288,000 miles in a second,‖ a velocity exceeding that of the light of Jupiter's Satellites.¶ Using this as a standard, all distances upon our globe are done away with, and we accordingly see in electricity the instantaneous messenger of thought for all distances and in every circumstance. Nevertheless the practical execution of a telegraph founded on this principle presents some difficulty. Francis Ronalds constructed one at Hammersmith in 1816, and published a description of the instrument in 1823. In his arrangement there were clocks which kept time employed at the stations and which were furnished with a light disc of ciphers in place of hands, and having twenty different signs towards their circumference. At the moment the proper sign

* Phil. Trans. Vol. XLV. for 1748.
† Young's Travels in France, 1784, Vol. I. p. 79.
‡ Voigt's Magazin, Vol. IX. Part 1.
§ Gauss and Weber Resultate d. Mag. Veriens, Vol. II. p. 14.
‖ Phil. Trans. 1834, p. 595.
¶ 192,500 miles in a second, Brewster's Optics, p. 2.

passes before the index the spark is discharged with an electrometer upon which the same sign is rendered visible on the clock at the other station. The disadvantage of being obliged to wait every time between each sign one gives till the disc has completed an entire revolution of course renders the plan impracticable. The old proposal of Cavallo* to employ the passage of the spark for a signal, would, when properly modified, be more applicable. It would be necessary, so that the signals may succeed each other with the utmost rapidity, and the arrangements be as simple as possible, that we should content ourselves with the discharging spark of a small coated plate. The sound which is always heard on the passage of the spark where there is a break in the chain would serve as an unequivocal signal, directly affecting the sense of hearing as already laid down by us in our general remarks upon convenient telegraphic intercourse. An electrical machine of moderate dimensions would continually furnish fresh charges at short intervals. It would not however be so easy to get over the difficulties attendant on the changes in electric excitation and the various hygrometric states of the atmosphere. But these difficulties are not insurmountable, and it is as yet by no means certain but what this plan, if followed up by a thorough knowledge of the subject, might not from its simplicity be brought into competition with any other principle, and this the more if due use is made of the important principle discovered as we have said by Winkler.

Of late years however we have become acquainted with a peculiar modification of electricity, which, affecting as it does with a reciprocal action several of the other powers of nature in a wonderful manner, even at first sight presents essential advantages for its application to the problem of telegraphic intercourse by means of electricity. We allude to the power of galvanism.

As long ago as in 1807 Sömmering erected in the apartments of the Academy of Sciences at Munich a galvanic telegraph, of which he has published a detailed description in the Philosophical Transactions of Bavaria.† He employed the energy of a powerful voltaic pile to bring about the decomposition of water by means of thirty-five gold pins immersed in an oblong glass trough, each of these pins being marked with a letter or number, and admitting of connexion with the pile by an isolated wire. The ascension of the air bubbles of

* Cavallo's Treatise on Electricity.

† Münchner Denkschr. d. K. Akad. d. W. für 1809, 1810. Math. phys. Classe p. 401.

the water decomposed was to serve as the telegraphic signal. It should be borne in mind that our knowledge of the effects of the galvanic pile was at that period but very limited. The discoveries of Oerstedt in 1819 from which we learnt that a magnetized needle can be deflected by a galvanic current, have opened a fresh field for the application of this power to telegraphic intercourse. Under this head may be comprised the hints for a galvanic telegraph thrown out by Fechner in his Manual of Galvanism,* and Ampères scheme of an electro-magnetic telegraph which has been executed on a small scale by the late Professor Ritchie.† In this plan however Ampère departs so totally from the desirable principle of simplicity that upwards of sixty metallic connexions would be required for his telegraph. The arrangement which Davy‡ has proposed, in which illuminated letters are shown by the removal of screens placed in front of them, appears also to be far too complicated. The experiments instituted by Schilling§ by the deflection of a single magnetic needle seem much better contrived, he did not however succeed in surmounting the mechanical difficulties that attend the question in this shape. The subsequent discovery of Faraday, according to which the generation of galvanic currents is reduced to the mere motion of multipliers placed near fixed magnets has very materially simplified the problem, inasmuch as the voltaic pile becomes thereby no longer necessary.

To Gauss and Weber‖ is due the merit of having in 1833 actually constructed the first simplified galvano-magnetic telegraph. It was Gauss who first employed the excitement of induction, and who demonstrated that the appropriate combination of a limited number of signs is all that is required for the transmission of communications. Weber's discovery that a copper wire 7460 feet long which he had led across the houses and steeples at Gottingen from the observatory to the cabinet of Natural Philosophy required no especial insulation was one of great importance. The principle was thereby at once established of bringing the galvanic telegraph to the most convenient form. All that was required was an appropriate method of inducing or exciting the current with the

* Lehrbuch des Galvanismus 1829, p. 269.

† Froriep's Not. Vol. XXVII. No. 6. p. 86.

‡ Mechanics' Mag. No. 754, p. 261; No. 756, p. 296; No. 758, p. 327.

§ Allgemeine Bauzeitung 1837, No. 52. p. 240.

‖ Gött. gel. Anz. 1834, p. 1273, and Schumacher's Jahrbuch, 1837, p. 38.

power of changing its direction without having recourse to any special contrivances for that purpose. In accordance with the principles we have laid down, all that was required in addition to this was to render the signals audible, a task that apparently presented no very particular difficulty, inasmuch as in the very scheme itself a mechanical motion, namely, the deflection of a magnetic bar, was given. All that we had to do therefore was to contrive that this motion should be made available for striking bells or for making indelible dots. This falls within the province of mechanics, and there are therefore more ways than one of solving the problem. Hence the alterations that I have made in the telegraph of Gauss and by which it has assumed its present form may be said to be founded on my perception and improvement of its imperfections, in harmony with what I had previously laid down as necessary for perfect telegraphic communication. I by no means however look on the arrangement I have selected as complete; but as it answers the purpose I had in view, and it may be well to abide by it till some simpler arrangement is contrived.

As an inductor or exciter I emp
whose construction, speaking in a g
those of Clarke, of London, the mul
my inductor is composed, consist of a
fine insulated copper wire ; and this
in order that the resistance offered b
pleting the circuit, even should it b
be but little increased. Of the ga
during the entire half-turn of the m
only a small portion is employed, a
maximum of its energy. By this m
current is but very short, an arrang
a manner can cause merely a mom
little magnetic bars employed for giv
to heighten the action of these indic
they are surrounded by powerful mul
magnets are so placed near these i
thereby brought back to their origi
that the induced current ceases, or in other words, as soon as the deflection has taken place: we thus are enabled to repeat signals in very rapid succession. The same indicator can be brought with ease to make five deflections in a second, succeeding each other as fast as the sounds of a repeater when striking. Hence if bells are placed at the proper striking distance from these indicators they will ring at every deflection produced, and as it is quite immaterial at what part of the

wire, completing the circuit, the multiplier containing the indicator is inserted, we have it in our power to produce the sign excited by induction at any part of the course the wire takes. Should it be desired that the indicator instead of producing sounds should write, it is merely required to adapt to one end of the little magnetic bar a small vessel filled with a black colour, and terminating in a capillary tube. This tube, instead of striking on a bell, thus makes a black spot upon some flat surface held in front of it. If these spots are to compose writing, the surface upon which they are printed must keep moving on in front of the indicator with a uniform velocity, and this is easily brought about by means of an endless strip of paper which is rolled off one cylinder on to another by clockwork. This is in a general way the construction followed in the telegraph erected here, a fuller account of which now would be out of place, as it may be found on referring to the appendix. As far as the employment of the telegraph is concerned, it may be fairly said to perform all that can be reasonably required of it. The excitation of the current is produced by half a turn of the inductor, and is equally available at all times. The sounds of the bells close to the person making the signals, and which being produced at the other station too are also audible there, become, by practice, intelligible as a language. Should they however be overheard or misunderstood, the communication presents itself simultaneously written down. This can be done with closed doors, without any but the parties concerned being aware of it; the communications may be made at any distances, and either by day or night without any appreciable loss of time. There is, therefore, every reason to be content with the performance of the instrument.

It is not however to be denied that the establishment of certain conditions indispensable to its action is nevertheless a matter of some difficulty. We allude to the connecting wire joining the stations.

It has been stated above that Ampère required more than sixty such wires, whereas thirty or so were sufficient for Sömmering. Wheatstone and Cooke* reduced their number to five; Gauss and, probably in imitation of him, Schilling, as likewise Morse† in New York, made use of but a single wire running to the distant station and back. One might imagine that this part of the arrangement could not be further sim-

* La France industrielle, 1838, April 5, p. 3.

† Mechanics' Magazine, No. 757, p. 332. Silliman's Journal for October, 1837. Annals of Electricity, &c., Vol. II. p. 116.

plified, such however is by no means the case. I have found that even the half of this length of wire may be dispensed with, and that with certain precautions its place is supplied by the ground itself. We know in theory that the conducting powers of the ground and of water are very small compared with that of the metals, especially copper. It seems however to have been previously overlooked that we have it within our reach to make a perfectly good conductor out of water or any other of the so called semi-conductors. All that is required is that the surface that its section presents should be as much greater than that of the metal as its conducting power is less. In that case the resistance offered by the semi-conductor will equal that of the perfect conductor; and as we can make conductors of the ground of any size we please simply by adapting to the ends of the wires plates presenting a sufficient surface of contact, it is evident that we can diminish the resistance offered by the ground or by water to any extent we like. We can indeed so reduce this resistance as to make it quite insensible when compared to that offered by the metallic circuit, so that not only is half tl
the resistance that such a circuit wou
by one half. This fact, the impo
erection of galvanic telegraphs speak
with another additional feature in wh
electricity. The experiments of W
already shown us that with frictiona
may replace a portion of the dischar
now known to hold good with respect

The enquiry into the laws of dispe
the ground, whose mass is unlimited
passage of the galvanic current, ap
replete with interest. The galvanic e
fined to the portions of earth situat
of the wire; on the contrary it canno
finitely, and it became therefore now
law that obtained the excitation of the
of the exciting terminations of the wi
sary or not to have any metallic co
carrying on telegraphic intercourse.

I can here only state in a general way that I have succeeded in deducing this law experimentally from the phenomena it presents: and that the result of the investigation is, that the excitation diminishes rapidly as the distance between the terminal wires increases.

An apparatus can, it is true, be constructed in which the inductor, having no metallic connexion whatever with the

multiplier, by nothing more than the excitation transmitted through the ground, shall produce galvanic currents in that multiplier sufficient to cause a visible deflection of the bar. This is a hitherto unobserved fact, and may be classed upon the most extraordinary phenomena that science has revealed to us. It only holds good, however, for small distances. It must be left to the future to decide whether we shall ever succeed in telegraphing at great distances without any metallic communication at all. My experiments prove that such a thing is possible up to distances of 50 feet. For distant stations we can only conceive it feasible by augmenting the power of the galvanic induction, or by appropriate multipliers constructed for the purpose or, in conclusion, by increasing the surface of contact presented by the ends of the multiplier. At all events the phenomenon merits our best attention, and its influence will not perhaps be altogether overlooked in the theoretic views we may form with regard to galvanism itself.

To sum up in a few words what are the results of what we have here brought forward respecting telegraphic communications, we see that, with the present arrangement of the apparatus, no principle can be brought into competition with the galvanic telegraph, but that the establishing the metallic connexion indispensable to its action, although now materially simplified, still presents great difficulties in practice. Indeed such a connexion is only practicable where it can be constantly watched, as, for instance, in the vicinity of railroads.

For very considerable distances without intermediate stations, galvanic or electric excitation must, on account of their rapidity, be always the best power to have recourse to. For lesser distances it yet remains open to enquiry whether, with proper modifications, some of the other methods we have pointed out would not be preferable, as they dispense with a metallic connexion.

*(To be continued, with two large illustrative plates, in the next Number.)*

---

## LVI. *Summary of new researches on the liberation of heat through friction. By* M. Becquerel, *President of l'Academie des Sciences for the year* 1838.*

Bodies are considered as formed by the connexion of an infinity of molecules or atoms surrounded by heat which is opposed to their immediate contact abstraction made by

* Translated by J. H. Lang, from the Comptes Rendus &c., No. 7, 1838.

every theoretical idea on its nature. When its quality increases or diminishes, the distance between the molecules becomes greater or less, and the size of the body experiences corresponding variations.

It is also admitted that these same molecules are subjected to an attractive force which tends to bring them nearer each other, and which is, consequently, opposed to the repulsive action of the heat. A third force is also introduced in the constitution of bodies, viz., the attraction of each molecule for the heat which surrounds the neighbouring molecules. The collective force prevailing over the two others, the body remains solid; if the heat increase, the molecules will at length acquire a certain motion, and the body becomes liquid. Finally, if the quantity of heat become sufficiently great, the body assumes the gaseous form. The molecules of the body being kept at greater or less distances in proportion to the reciprocal action of the heat, and attractive force, they must be separated by interstitial spaces in which the phenomena of light, heat, affinity, and molecular attraction operate. Hence, it is in these spaces that the imponderable agents contend with the material principles of the body.

Heat should in this have the principal place, for, according to its intensity and mode of action, it produces light and electricity, and sets in motion chemical affinities. Hence, it is evident we cannot too deeply study the properties of this agent, in its relations to the particles of bodies, if we wish to discover its immediate influence in every thing which concerns the natural phenomena of the most elevated order. These considerations suggested the idea of a series of experimental researches, which have produced some new results of which I shall endeavour to give a sketch without, as much as will be in my power, entering upon the technical details, which would be difficult to comprehend in a rapid lecture.

Let us take a body that is in equilibrio of temperature with the surrounding medium. If by any means we disturb this body, so as to make its molecules lose their natural position of equilibrium, it is very evident all the imponderable agents which are in the intermolecular spaces will be set in motion: whence there results a number of phenomena that the philosopher tries to analyze by the assistance of means which science places at his disposal. We shall first dwell upon the effects of heat when friction is the mode of disturbance employed.

We know that when two bodies are rubbed together, heat and electricity are disengaged; are or are not these two effects, which are concomitant, dependant on each other?

We will answer this hereafter; for the present let us consider the effects of heat.

All we know of the production of heat by the mutual friction of two bodies is reduced to this: the two bodies become heated, and the quantity emitted is sometimes so great that it is sufficient to inflame combustible bodies. Thus it is that a wheel, turning rapidly on its axle, takes fire, and that the savage, with an address and dexterity which we do not possess, is able to light two pieces of dry wood by rubbing them briskly together.

Every thing leads to the belief that the effects produced are due to the motion of vibration, impressed by vibration on the molecules, which the following facts tend to prove.

When an alloy, composed of one part iron and two antimony, is subjected to the file, it immediately throws off bright sparks, which shows that the temperature is raised even to a state of white heat. The blow of the steel on a flint produces a similar effect.

M. De Rumford* in boring a cannon, placed vertically, obtained sufficient heat to boil water, placed in a conveniently situated cavity. This is almost all we know of the liberation of heat by friction: hence, we are totally ignorant what part each body takes in the production of this phenomenon, as regards its nature and the state of its surface.

To determine how each body intervenes, we must be able to remove the causes which hide the effect we have in view; unfortunately we cannot effect this thoroughly. In fact, when we rub two bodies more or less briskly together, without causing the contact to cease, there is evidently a transmission of heat from the one to the other. The quantity transmitted by each of them depends on the conductibility of the body, its capacity for heat, and the state of its surface. On the other hand, the heat liberated in a body cannot be immediately tried, previous to its transmission to the other body, by ordinary thermometers, since their indications are not instantaneous. However, it is possible to operate under circumstances which permit the removal of several of the difficulties mentioned above: we are then led to a series of facts of which we are about to speak.

The apparatus intended for the observation of these facts is composed of a thermo-electric pile, in connexion with an excellent multiplier. Its sensibility is such, that the difference of about $\frac{1}{100}$ of a degree between the temperature of the two faces of the pile, will cause the magnetic needle to deviate so that the angle may be appreciable.

* Count Rumford. Edit.

To reduce the question to the most simple expression possible, we took bodies of the same nature, bad conductors of heat, equal in all their dimensions and differing only in the state of their surfaces. These bodies were conveniently fixed to glass stands. The rubbed surfaces were each placed in contact with one of the faces of the pile; when these two surfaces have the same temperature, the magnetic needle remains steady while the two thermo-electric currents being equal, and having contrary directions, destroy one another. But when the temperatures are not the same, the magnetic needle immediately deviates, and the angle of deviation measures the difference of temperature. The friction is produced with a briskness and pressure regulated by convenient apparatus, so that its intensity may be always known: the two bodies are quickly separated from each other, and immediately subjected to experiment.

There are the means of experimentalizing; let us now pass on to the results.

We commenced by trying the effect produced on the magnetic needle by the contact of one of the rubbed surfaces with one of the faces of the pile; an effect due to this face being heated.

Experiment proves that whatever may be the nature of the disc rubbed, be it a conductor or not of heat, the time the needle takes to attain its maximum deviation, provided this deviation do not exceed 60°, is always 10.″ For deviations from 60° to 70° is 9½″, and 9″ for deviations from 75° to 90°.

The magnetic needle here acts as a pendulum which oscillates under the action of weight among small amplitudes, since the deviations are isochronic; but, nevertheless, with this difference, that in the pendulum, when the amplitude of oscillation increases beyond a certain limit, the time of oscillation increases equally, while the contrary takes place in the experiments we have described, viz., that the time diminishes in proportion as the amplitude increases from 60° to 92°. This result is bound to the propagation of heat and electricity in bodies.

We will now take two bodies of the same nature, equal and disposed, as before. For example, two discs of cork, of which one has a smooth, the other a rough surface. If we rub them together in a regular and determined manner, and present them simultaneously to the two faces of the thermo-electric pile, the magnetic needle is immediately deviated, and the direction of the deviation indicates that the disc with the rough surface has taken more heat than the other, and that in a proportion which varies with the rapidity of the

friction. The same is the result when we rub a piece of polished against an unpolished piece of glass. Under the circumstances which we have operated, the first has taken half as little heat as the second. Hence, we see that the absorbing power of bodies influences the liberation of heat by friction: however, this rule is not general, for white satin takes more heat than black which has a greater absorbing power.

If we submit to experiment, bodies of a different nature, we obtain the following results: 1st, polished glass and cork; the first takes more heat than the second in the proportion of 34 to 5. 2d, unpolished glass and cork; the relation of temperature is 40 to 7. 3d, silver and cork; the first is heated above the second in the proportion of 50 to 12. 4th, caoutchouc and cork; the temperature of the first is to that of the second as 29 to 11, and so on.

From the numerous results we have obtained by the friction of bodies of different natures, we have not yet been able to draw from them any simple laws, considering the different causes which concur to the general effect. It only appears that the nature of the body, and the abstraction made by the conductibility, excercise an influence which the state of the surface does not always destroy.

We have not hitherto been able to discover the true causes of this influence, which depends on the nature of the bodies, and probably on the arrangement of their molecules; but it is even much to have distinguished it by experiment, since it gives us an element more than the theory of heat will be able to take henceforth in consideration. If we now seek for the relation which exists between the production of heat and that of electricity, by the mutual friction of two bodies, the following are the consequences deduced from recent experiments; the displacement of the parts of the surfaces rubbed, always causes a liberation of heat and electricity, two effects which are in a reciprocal dependence; this dependence is as yet so hidden that it is impossible to affirm whether one precedes the other, and *vice versâ;* at present we can only make conjectures on head; conjectures which tend to show that heat proceeds from electricity, when the bodies are of the same nature, bad conductors of heat, and differing only in the state of their surfaces; that which is heated the most takes the negative electricity, and *vice versâ.* When the bodies are different the effects become very complex, and can only be understood by having the results before our eyes.

New facts permit us to extend to light, the relations discovered between heat and electricity: they are furnished

by phosphorescence. We know that this phenomena is manifested whenever the particles of bodies, which are bad conductors of electricity, are disturbed by percussion, friction, heat, light, an electric shock, or even when they are decomposed by a chemical action. These are precisely the causes which also liberate electricity; but the phenomenon being molecular, the recomposition of the liberated electricities around the molecules, causes an infinite number of small sparks, which together produce a faint light similar to phosphorescence; whence we may conclude that phosphorescence has an electric origin.

We were ignorant from whence the phosphorescence in Lampyres and Infusoirds proceeded, and whether it also could have an electric origin; some important experiments of M. Ehrenberg have shown it to us. This skilful physiologist has just studied, with particular attention, the light emitted in a dark place by Infusoires and Annelides, which render the sea luminous in certain countries, and especially when a light breeze agitates its surface. Having placed some water, containing these animalculæ, on the object-table of his microscope, he was very much astonished to find the faint light which surrounded them, was only the union of a great many small sparks which proceeded from all parts of their bodies, and chiefly from the bodies of the Annelides. These sparks, which succeeded one another with great rapidity, had such a resemblance to those we observe in electric discharges, that M. Ehrenberg did not hesitate in establishing their identity with them. He is also certain that the light emitted is not due to any particular secretion, but to a spontaneous act of the animalcule: and it is shown as often as we irritate it, either by mechanical or chemical means, viz., by agitating the water or pouring either acid or alcohol therein. This is another analogy to the torpedo which only gives its discharge when irritated. In the animalculæ, as in the torpedo, the discharge recommences after a certain interval of repose. May we not conclude from this similitude of effects, under the same circumstances, that there is an identity in the causes? We know beyond a doubt that this cause in the torpedo is electricity; we must therefore admit that this also produces the phosphorescence of Infusoires and Annelides. It is very remarkable that luminous or other phenomena, which depend upon electricity, are so much stronger as the animals are smaller. It appears that this profusion of electric fluid, emitted only by beings of an inferior order, is destined to perform some other functions in those of a superior order,

May we not after this conclude, as M. Berzelius and other philosophers have advanced, that the light emitted in combustion, which gives place to so great a liberation of electricity, also results from the discharge of an infinite number of small sparks produced by the combination of the combustible burning bodies.

Hence we see that the relations which connect light, heat, and electricity, are every day extending and convincing us that these three agents which preside over the constitution of molecular bodies, proceed, by all appearances, from one principle of ethereal nature, spread over space and all bodies.

---

## LVII. *Organic Chemistry. Note on the Formométhylal. By* M. MALAGUTI.*

The formométhylal, discovered by M. Gregory, is a body of great importance, for it is connected, by its composition, with one of the most creditable theories of the æthers, giving it one of the strongest arguments.

The formométhylal is considered as a formiate of tribasic mythylic æther, analogous to the acetal which is also considered as an acetate of trebasic sulphuric æther.

In order to see if the formométhylal had the constitution which was attributed to it, I decomposed an atom of it in the expectation of obtaining an atom of the formic acid; but I only obtained half an atom of pyroligneous spirit, and also a particular body endowed with several characters which, for want of a profound examination, might be confounded with the formométhylal from which it is derived.

The composition of this new body which I call methylal, is exactly represented by $C^{12} H^{16} O^{4}$, and the density of its vapour is 2.6 or 4 volumes, while the formula of the formométhylal is $C^{16} H^{20} O^{6}$, and the density of its vapour is 2.4 or 6 volumes.

We see by subtracting the formula of the methylal from that of the formométhylal, there remains half an atom of formiate of methylene.

$$\begin{array}{l} C^{16} H^{20} O^{6} = \text{formométhylal.} \\ C^{12} H^{16} O^{4} = \text{methylal.} \\ \hline C^{4} \;\; H^{4} \;\; O^{2} = \tfrac{1}{2} \text{ atom formiate of methylene.} \end{array}$$

The methylal is limpid, has the same smell as the formométhylal, and requires about three volumes of water to dissolve

* Translated by J. H. Lang, from the Comptes Rendus &c., No. 9, 1838.

it; potassa separates it from its aqueous solution. It is soluble in alcohol, and at + 42 deg. c., under the pressure of 0.761 millim. Water being 1 it weighs 0,8551 at + 17 deg. c.

Notwithstanding the striking relation between the methylal and formométhylal, notwithstanding the density of this latter body, which sanction, if I may so speak, this relationship so evident and so simple, the formométhylal is only a mixture of the methylal and formiate of methylene.

If we distil some formométhylal, whose composition has been verified by very careful analysis, and divide the product of the distillation into fractions, we shall find that each fraction no longer presents the same composition.

The formométhylal which I have used in my experiments has given by analysis numbers which are confounded with those of calculation. On analyzing the first product of its distillation, I found in it 2.3p. 100 less carbon; and on the contrary, I found 1.1p. 100 more on analyzing the last product. As regards the nature of the methylal, my experiments are not sufficiently advanced to be able to decide, but I can say with certainty that the methylal, that by the action of certain oxidizing bodies, is converted into formic acid.

I cannot say whether pyroligneous spirits be formed at the same time or not, as my method of experimenting prevents me from insulating or even recognizing this body.

But whatever may be the nature of the methylal, it is as true as in formométhylal, far from being a tribasic æther as it has been supposed, it is only a mixture of formiate of methylene, and a substance which cannot be compared to the compound æthers.

As soon as I have completed the researches I have undertaken on the nature of the acetal, I shall have the honour of submitting to the judgment of the Academy, not only the results relative to this latter body, but also the whole of the details, an abridgment of which I am now permitted to present.

---

*Organic Chemistry. Note on the products obtained by* M. Masson *from causing chloride of zinc to act on alcohol.*

M. Masson, an old pupil of the Normal School, and professor at Caen, had addressed to the Academy a memoir on the formation of æther which results from the action of chlorure of zinc on alcohol. He has just completed this work by some analyses of the products that he had obtained; made at the request and in the laboratory of M. Dumas, one of the

commissioners appointed to examine the first part of these researches.

M. Masson by distilling chloride of zinc with alcohol at about 160°, obtained an oil, the production of which did not cease until 200°. He compared it with sweet oil of wine, remarking that it could be separated into two parts of unequal volatility.

From another examination to which the author submitted these two products, the result was, says M. Dumas, that the least volatile ceases at about 300°, when it has lost all foreign matter. This product possesses all the characteristics of light sweet oil. Its analysis gave,

| | |
|---|---|
| Hydrogen | 12.8 |
| Carbon | 88.1 |
| | 100.9 |

Which corresponds with $C^{32} H^{28}$ or with $C^8 H^7$. This analysis agrees with neither that of M. Hennel, nor of M. Sérules. It is nearly similar to that which M. P. Boullay and myself have given.

The most volatile product ceases at less than 100°, is very fluid, and its smell resembles that of naphtha; its composition is remarkable. It is the carburet of hydrogen, the most hydrogenerated liquid known. It contains

| | | |
|---|---|---|
| Hydrogen | 15.7 | 15.7 |
| Carbon | 84.5 | 84.7 |
| | 100.2 | 100.4 |

Whence we take for our formula $C^8 H^9$. But from the density of its vapour, which is equal to 4.18, we may represent by $C^{32} H^{36}$.

Thus, there would be in M. Masson's oil two carbures of hydrogen which, by uniting, would reproduce bicarburated hydrogen from which they are derived.

$$\text{Volatile carbure} \ldots \frac{C^{32} H^{36}}{4} = C^8 H^9 \ldots 4.1$$

$$\text{Sweet oil of wine} \ldots \frac{C^{32} H^{28}}{2} = C^{16} H^{14}$$

---

*Organic Chemistry. On the composition of Sugar of Gelatine; and on the nitro-saccharic acid of Braconnot. By* M. Boussingault.

Sugar of gelatine prepared in the usual manner by means of the reaction of sulphuric acid on strong size, according to

M. Boussingault always contains a soluble quantity of saline matters; so that after its combustion it leaves from 2 to 11 per cent ashes: but we may obtain it in a pure state by the decomposition of the soluble salt which it forms with baryte. For this purpose, a solution of sugar of glue is made to boil for some time with a milk of baryte; it does not disengage itself of ammoniac; it is filtered, and the baryte separated completely from the filtered liquor by means of sulphuric acid. It is afterwards evaporated to a pellicle and then the sugar quickly crystallizes.

Sugar of gelatine is rather more soluble in water than sugar of milk; like the latter it cracks under the teeth; its sweetness is rather intense and leaves a disagreeable taste behind. Once dried in a dry vacuo, at the ordinary temperature, it loses no more water at 130°.

The mean result of seven analyses, the details of which may be found in M. Boussingault's note, gives the following rough formula for the composition of sugar of gelatine.

$$C^{12} H^{31} O^{11} Az^{6}.$$

By causing a solution of sugar of gelatine to boil over powdered litharge, we obtain a liquid, which, filtered, presents an alkaline reaction, and sufficiently evaporated forms by cooling a crystalline mass.

The mean of four analyses has given for this salt of basic lead, a composition which may be represented by the formula

$$C^{12} H^{27} O^{9} Az^{6}, 3 (PbO),$$

which compared with the preceding, would indicate that by combining with three atoms of oxide, the sugar of glue lost two atoms of water. Under this supposition the atom of anhydrated sugar would weigh 2517.0.

If we consider the combination of lead as neutral, the atom of the sugar will be reduced to 835.7 and the anhydrated sugar would be

$$C^{4} H^{9} O^{3} Az^{2}.$$

But then the uncombined sugar must be represented by the preceding formula + $\frac{2}{3}$aq.

The *nitro-saccharic acid* prepared by M. Boussingault, according to the process indicated by M. Braconnot, but with sugar of pure gelatine and with certain precautions indicated in the note, has given a composition which may be represented by the following formula:

$$C^{4} H^{13} O^{10} Az^{1}$$

In the nitro-saccharate of copper prepared by the direct combination of acid with very divided oxide, then dried at

130°, the acid has been found in the same state as before the combination.

At this point of desiccation, the salt is of a pale blue; if we expose it to a higher temperature it takes a decidedly green tint. This colour appears at about 150°, and we perceive some water in the upper part of the tube containing the salt. At 180° or 182° the detonation takes place.

The salt, previously dried at 130°, and retained for several hours in vacuo at a temperature of 165°, loses 0.1771 of water in 100 parts.

The results of the analysis of the nitro-saccharate of anhydrated copper, permit the adoption of either of the two following formula for the composition of this salt:

$$C^2\ H^3\ O^3\ Az^2\ CuO,\ \text{or}\ C^4\ H^5\ O^6\ Az^4 2\,(CuO):$$

but, says the author, it seems preferable to adopt the last of these formula, because it enables us to establish a relation with the sugar of gelatine.

| | |
|---|---|
| The nitro-saccharic acid being | $C^4\ H^5\ O^6\ Az^4$ |
| If we take away the azotic acid | $O^5\ Az^2$ |
| There remains | $C^4\ H^5\ O\ Az^2$ |
| The sugar of glue in the salts being | $C^4\ H^9\ O^3\ Az^2$ |
| | $H^4\ O^2$ |

which shows that by combining with the azotic acid, sugar of glue loses more water than would be necessary for passing to the state in which it is found in the salts.

## LVIII. *Memoir on a new electric condenser*. By M. Peclet.*

The new condenser is composed of three unpolished glass plates, which have been carefully ground together and completely covered with gold leaf fastened with albumine. One of these plates which I shall call A, is fixed to a common gold leaf electrometer; its upper surface is covered with varnish. The second which I shall distinguish by B, is placed on the first, and varnished on both surfaces. A small copper wire carefully gilded and unvarnished is fixed horizontally to a point in its circumference; it has at its centre, similar to the moveable plate of common condensers, a glass handle for moving it with. Lastly, a third plate C, is placed on this last with a hole in its centre through which passes the wire of the

* Translated by Mr. J. H. Lang, from the Comptes Rendus, &c. No. 9, 1838.

plate B; the plate C is only varnished on the lower side and its central orifice is supplied with a glass tube which surrounds the wire of the plate B, but of a less height.

The following is the method of using this apparatus. The upper plate is touched with the metal of which you wish to discover the action on the gold; the plate B is put in connexion with the ground; we suppress this connexion, raise the plate C and touch the A; this is repeated a certain number of times; at last by means of the wire on the plate B we raise B and C together; the gold leaves of the electrometer diverge, more or less, in proportion to the number of contacts.

The case which encloses the gold leaves is formed of parallel glasses, and placed on a trivet provided with a screw, on one side, a vertical plate pierced with a small hole, and on the other a portion of a circle divided vertically, whose centre is the same height as the hole in the plate and the upper extremity of the gold leaves; by looking through the hole in the plate we observe the deviation.

To give an idea of the power of this apparatus I shall relate two series of experiments. By touching the upper plate with an iron wire, after 1, 2, 3, 4, 5, and 10 contacts the gold leaves diverged $9\frac{2}{3}$, 20°, 25°, 31°, 41°, and 88°.

By touching the upper plate with a platina wire, a single contact produced only a slight deviation, which was increased to 15° after three, and to 53° after twenty contacts.

The experiments with the platina were made by employing a platina wire, which had just been reddened in an alcohol flame, and washing my hands in distilled water. I was convinced beforehand, by making a great number of successive contacts in which I touched the upper plate with my finger, that the plates concealed no electricity.

The novel fact of the development of electricity by the contact of gold and platina, has also been directly proved by means of a simple condenser of extreme sensibility obtained by giving a sufficient thickness to the coats of varnish and making their surfaces perfectly smooth.

By means of the double and common condensers I have discovered that all the metals on which I have operated were positive with regard to gold, and these metals arranged in the order of their electro-motive power with regard to gold are as follows:

| | | |
|---|---|---|
| Zinc | Bismuth | Copper |
| Lead | Antimony | Silver |
| Pewter | Iron | Platina. |

The effects produced by bismuth, antimony, and iron, differed so little from each other, that I could only classify them by taking the means of a great number of experiments.

It is evident from the arrangements of the apparatus that the quantity of electricity liberated, which causes the gold leaves to diverge, is proportional to the number of contacts; but we find from numerous experiments that as far as about 20° the deviation is proportional to the number of contacts; thus within this limit the deviation is in proportion to the quantities of electricity. It would be easy to make a table which should give the quantities of electricity corresponding to the deviations which exceed 20°, since these quantities are proportional to the number of the contacts.

The simple condensers or multipliers cannot however determine the relations of the effect produced by the contact of gold and different metals, since these proportions vary considerably with the thickness of the coats of varnish on the plates, of which I have satisfied myself by comparing the results of experiments made with different apparatus.

The instrument that I have the honour to present to the Academy, being of a sensibility somewhat indefinite, offers to philosophy a new means of investigation, which I hope will contribute to throw some light on the singular phenomena, which are produced by the contact of bodies.

---

## LIX. *On the different phenomena of electricity at rest, and electricity in motion. By* Charles V. Walker, Esq.

1. A paper was lately read before the Society, wherein the author endeavoured to prove, that homogeneous *attraction* and not *repulsion* exists between the particles of electricity.* He has candidly invited the opinions of others; and, though adducing an experiment, the experiment to which the present communication will mainly refer, that at first sight seems to support his hypothesis, yet he intimates a readiness to abandon his position should it be found untenable.

2. It may be urged, that the time for pursuing this sub-
the debate, which followed the reading of the
to a certain extent, it was. But, I am sure
ded me, that when a new opinion or experiment
d, it is not always possible to enter unprepared
argument. An *opinion* may be formed; yet, oftimes the peculiar nature of the fact requires a train of thought, not to be pursued conveniently at a moment's notice.

3. The authors of papers will, therefore, not conceive that the length of a debate is a measure of our attention to, and opinions upon, their communications.

* The paper alluded to is Mr. Griffin's. See Annals of Electricity, &c. Vol. III. p. 126. Edit.

4. But our ideas may assume a more tangible form when meditated more at leisure, and it is greatly to be desired that these our more *matured* opinions should be reduced to order—should be tested by experiment—and should be brought before the Society. I would that, comet-like, every paper read here should leave a train, if not of *light*, yet surely of *reflection*, in its wake.

5. The main purpose of a Society being, that the mutual talents of members should be directed to any subject submitted to its consideration, it would tend greatly toward the object and interest of these our evening meetings, were all present to consider themselves as concerned to determine the truth or fallacy of those propositions, which are advanced here, and so to enter into the spirit of papers read before them, as to be prepared now or after with their yea or nay, their assent to or dissent *from the conclusions* adduced.

6. Conceiving the experiments offered in favour of homogeneous *attraction* as inconclusive, I have investigated them, or rather *one* of them, (the others being but modifications of this one) and have ventured to show the grounds of objection against their being a philosophical testimony in support of the author's hypothesis.

7. The experiment referred to is this:—

By suitable arrangements, two streams of electricity were made to pass in vacuo from two points, say A and B, to a ball, C. It was invariably found that in their passage they converged, and entered the ball, C, in one compound stream, forming an appearance resembling the letter Y.

8. It would seem, that, unless h
a fundamental principle of electricity
have continued independent, pursuin
would have converged till they had
ing in their passage a resemblance t
the other hand homogeneous *repuls*
to prove) the fundamental principle
diverge in their passage and present

9. But it will occur to the atte
important element in this experime
viz., *motion*. An *important* element, because it leads to a branch of the science, unconnected with the matter in debate, to electro-*dynamics*, the laws and phenomena of which are entirely distinct from those of electro-*statics*.

10. In investigating the properties of the fluid, *per se*, it should first be accumulated, then left in a state of *rest*, and then examined. The result of such an examination will, by

no means, correspond with the results obtained from the experiment now under consideration.

11. The distinctive phenomena of the two states are so well known, that they scarcely need be mentioned; yet, for the sake of method, I will avail myself of the following passage from Dr. Faraday's Researches (Third Series, § 267).

12 "The various phenomena exhibited by electricity may, for the purposes of comparison, be arranged under two heads: namely, those connected with electricity of tension, and those belonging to electricity in motion. This distinction is taken at present not as philosophical, but merely as convenient. The effect of electricity of tension, at rest, is either attraction or repulsion at sensible distances. The effects of electrical currents may be considered as 1st, evolution of heat; 2d, magnetism; 3d, chemical decomposition: 4th, phisiological phenomena; 5th, spark."

13. With the experiments, productive of these effects, all are so well acquainted, that it would be tedious to repeat them. They are mentioned merely to show *generally* the characteristic differences between the two conditions; they remind us how largely *motion* is concerned in producing the peculiar phenomena of electricity, and how very confined is its agency, (even though accumulated) when *at rest*.

14. Our present object simply requires us to enter into those particular experiments wherein *attraction* and *repulsion* are concerned:

Let the conductor of a machine, or a wire attached to the conductor sufficiently long to reach beyond the atmosphere of the cylinder, be connected with the earth. In this case, however powerfully the machine may be in operation or however continuous and copious a current be passing along the wire, (providing it is not more than the wire is fitted to conduct) yet light substances will remain unaffected by its presence, although placed in the immediate vicinity.

15. But this is not the case when the fluid is *at rest*. For if, instead of suffering it to pass off to the earth, it be permitted to accumulate in a Leyden jar, on testing its effects, by approaching a pith ball, the latter will be instantly *attracted;* and, if insulated, as instantly *repelled*.

16. This indicates an important difference between *rest* and *motion*. The *one* condition exercises a powerful disturbing influence upon neighbouring bodies; the *other* is without influence; at least so far as *attraction* and *repulsion* are concerned: and attractive and repulsive influence alone are we now examining.

17. There is one case, to which I will briefly refer, wherein both conditions, *motion* and *rest*, are involved. So far, as my own information extends, attention has not been directed to it by any writer on the science. It is when a Leyden jar is in connexion with the prime conductor of a machine, while the cylinder is revolving.

18. In this case, it would seem that, so soon as the machine is put in motion, a current of fluid passes along the conductor to the jar. It there spreads itself in a thin layer, (if I am allowed the term) over the inner coating, over the surface of the wire, and over the surface of the conductor, as forming part of the wire. As the machine continues in motion, the tension of this accumulated fluid increases, by the addition of fresh layers beneath the former; so that while electricity is in *motion* beneath the surface, other electricity is at *rest* on the surface. And this latter as might be expected exerts an attractive and repulsive influence over light bodies, the other being as before inactive; but this is digression.

19. The comparative experiments bearing more immediately on the very point in debate are these:—

Let the negative wire of a powerful voltaic arrangement be in communication with the outer coating of a Leyden jar, while the positive wire is brought suddenly to the knob; the jar will receive a charge, sufficient to affect an electroscope, the gold leaves of which will *separate*, indicating *repulsion*.

20. Let an apparatus of two parallel and moveable wires, delicately poised, be so placed in connexion with the same battery, as that, when the circuit is complete, the current shall pass along both. If while traversing wire A, in one direction, it traverse B in the opposite, the wires will exhibit signs of *repulsion*; but if the current pass along both in the same direction they will be found to approach, evincing signs of *attraction*. To this circumstance then would I chiefly allude: that the same voltaic electricity which when accumulated in a jar, and thence distributed into the electroscope, caused the gold leaves to diverge, is found (while passing along fit apparatus in the same direction) to cause the wires to converge; being in the former case *at rest*; in the latter *in motion*.

21. And it is precisely under similar circumstances that the experiment in question was made:

Two streams of electricity were passed in the same direction from points A and B, to a common destination, C.

22. In obedience to laws observed in other cases, it might be expected that they *would* (as they do) evince homogeneous *attraction*, (the only difference being in the nature of the

conductor, in the one instance wire, in the other vacuum). But it is by no means legitimate to conclude from this, that homogeneous attraction belongs to electricity *per se*; but rather that it belongs to electricity *in motion*.

23. Lest it be objected that the experiment in question was made with *frictional* electricity, and that *voltaic* is selected to illustrate it, I would observe that the selection is merely made because experiments with the latter, when in motion, are more generally known. But, to show that both are similar in their laws and effects, I will again avail myself of an authority to which all will pay respect. (Dr. Faraday, Third Series, § 360.) The general conclusion which must, I think, be drawn from this collection of facts is, that *electricity, whatever may be its source, is identical in its nature.* The phenomena in the five kinds or species quoted,* differ not in their character, but only in degree."

24. There is then inserted "a table of the experimental effects common to the electricities derived from different sources;" by referring to which, it will be noticed that all the effects of common electricity are produced by voltaic, and vice versa.

| | Physiological effects. | Magnetic deflection. | Magnets made. | Spark. | Heating power. | True chemical action. | Attraction and repulsion. | Discharge by hot air. |
|---|---|---|---|---|---|---|---|---|
| Voltaic electricity. | × | × | × | × | × | × | × | × |
| Common electricity. | × | × | × | × | × | × | × | × |

25. In addition to this, and as if to supply every evidence of identity, the very experiment *in question* has been performed; wires have been arranged; electricity from the machine has been passed along them; dissimilar currents have repelled; and similar have attracted.

26. From these observations, I think it will appear that the experiment in question is not of a kind to be admitted as evidence in support of the author's hypothesis, and, therefore, so far as the theory of homogeneous attraction depends on this, so far is it void of foundation.

27. In addition to the above, seventy or eighty experiments were made, (with the same object in view) by passing charges of electricity over the surface of cards capable of receiving a permanent mark thereby; but as these are all liable to the

* Viz., 1, voltaic; 2, common; 3, magnets; 4, therm.; 5, animal.

same objection, in that the electricity under examination was *in motion*, it will not be necessary to allude to them further.

28. In reference to the experiments in question, the author says: "If the above observations and experiments are not considered sufficient to establish the existence of the power of homogeneous attraction in electricity, I have other experiments devised, varying the above methods."

29. Should these other have been made with the fluid *at rest*, the results may be admissible: and, should they bear with them sufficient evidence, although wedded to the opinion of homogeneous *repulsion*, I will willingly resign it.

30. The principal design of the present communication having been to advert to the fact, that electricity *in motion* and electricity *of tension* are different in their effects, I have not thought it needful to investigate the *cause*; for whatever the *cause* be, the result is certain, is constant.

31. In conclusion I would observe that as these observations have been made in support of a theoretical notion, some may urge, that so long as we are acquainted with effects and the modes of producing them, it is of little consequence whether they depend on this or on that theory; whether, for instance, the phenomena of electricity depend on two fluids or on one; or whether they originate in a series of expansions and contractions. For my own part I must candidly confess that I would at once close all my electrical books, lock up my apparatus, and withdraw my name from this Society, unless I hoped to derive something more than mere outward signs; something more than mere visible effects and the modes of producing them, from devoting my time and thoughts to the study of this science.

32. The intellectual electrician is not satisfied with the shock, the spark, the heat, the decomposition, the deflection of the magnet, the mere *signa* operandi; he would investigate further, and endeavour to trace the *modus* operandi in each instance; he would penetrate behind the veil, and seek those mysterious laws which regulate the whole. By the induction of particulars, he would (as a good statesman) frame such a general code, as should suit every individual case; would reduce this code into so small a compass, that the whole statute book could be registered in the mind: and would render it so simple, that its application to every case might be readily perceived and as readily effected.

*Kennington Grammar School,*
*August* 31*st*, 1838.

## LX. *On a new compound of Carbon and Hydrogen.* By WILLIAM MAUGHAM, Esq. *Lecturer on Chemistry, at the Royal Gallery of Practical Science, Adelaide Street, London, &c. &c. Communicated in a letter, dated June 21st, 1838, addressed to the Honorary Treasurer of the London Electrical Society.*

Read 3d July, 1838.*

Sir,

During the year, 1834, in consequence of certain experiments that were made with different kinds of artificial lights, at the Royal Gallery of Practical Science, by Messrs. Watkins, Gardner, Wilkinson, and myself, I was induced to pay considerable attention to the light produced by voltaic electricity, through the medium of charcoal points, for the purpose of rendering that light continuous and of the same intensity for any given period. I found that by increasing the number of the pairs of points, a corresponding number of lights might be accordingly produced of *apparently* the same intensity; the several pairs of points being placed within the voltaic circuit and insulated upon glass rods fixed in a piece of board. On bringing the points, thus arranged, in contact, and then separating each pair at the same instant (the apparatus being so contrived as to admit of this being done), sparks were produced simultaneously at the several interruptions. This experiment it is to be understood was suggested by my friend Mr. Williams, who very ingeniously contrived the necessary insulating apparatus alluded to. Whilst repeating this experiment with M. de la Rive, it was proposed by him that one of the pairs of points should be placed in water, when the spark, as might have been expected, was visible in that liquid, and at the same time a spark was visible at each of the other pairs of points out of the water. The experiment was continued for about a quarter of an hour, and as it was proceeding, a very peculiar odour, arising from the water in which the electrical light was produced, became perceptible.

On reflecting upon this afterwards, it struck me that some change must have taken place between the water and the charcoal during the passage of the electricity. To satisfy myself of the truth of this, I prepared a battery of about sixty pairs of four-inch plates arranged on Wollaston's principle; to the ends of the two electrodes I affixed two pieces of char-

* From the Transactions of the London Electrical Society.

coal by means of platinum wires of sufficient length to allow the charcoal to be conveyed into a glass vessel containing distilled water. A glass tube was next filled with distilled water and then inverted in the water into which the charcoal points were to be immersed. On bringing these points together under the water so as to produce the electrical light with as little interruption as possible, the water underwent decomposition and the glass tube was soon filled with gas. When flame was applied to the mouth of the tube no explosion took place as when a mixture of oxygen and hydrogen gases is obtained by decomposing water when the action is electrolytic; but the gas burned silently with a flame similar in colour to that by which the flame of carbonic oxide is distinguished. Suspecting, therefore, carbonic oxide to have been produced, I passed 2 volumes of the gas obtained as above into an eudiometer with 1 volume of pure oxygen; the mixture was made over mercury, and lime water was then introduced above the mercury in the eudiometer. No change was observed in the lime water, but after passing an electric spark through the gases a gradual condensation of the whole took place and the lime water was rendered turbid, carbonic acid gas having evidently, in the first instance, been produced. If the spark be passed before the lime water is added the same condensation takes place as when we explode a mixture of carbonic oxide and oxygen, and lime water being then introduced it becomes turbid with a gradual absorption of the whole of the gas. A *very little* residual gas was sometimes observed, which disappeared after the introduction of a crystal of proto-sulphate of iron, showing the residual gas to have been oxygen added in excess.

It is to be remarked that on passing lime water to the gas obtained as above described, I have observed that it is sometimes very slightly changed, *but this certainly does not always take place*. It would, therefore, appear that a trifling quantity of carbonic acid gas is occasionally formed along with the carbonic oxide. Neither hydrogen nor oxygen can be detected. Therefore should these gases be found by any one else along with the carbonic oxide, it will be owing to the action becoming electrolytic in consequence of the spark not being continuous during the operation, but when the spark is produced the action is not electrolytic, and it is only then when the carbonic oxide is formed.

When water, therefore, is acted upon as already described, carbonic oxide is produced by carbon combining with its oxygen, and at the same time a peculiar compound is formed by another portion of the carbon uniting with the hydrogen of

the water, no hydrogen as has already been shown passing off in the gaseous state.

This compound, which does not appear to have been before observed, imparts a very peculiar and unpleasant odour to the water in which the charcoal points have been immersed whilst under the influence of the battery, but the process should be carried on for about a quarter of an hour to be satisfactory, during which period the spark from the charcoal points should be continually visible under the water. The water thus impregnated with the new compound is of an oily nature, which may be seen by pouring it into a clean tube, then emptying the tube and holding it up to the light. When kept for some time the liquid loses its odour and there is a precipitate of carbon. This spontaneous change takes place whether it be exposed to the air or kept in a stoppered phial.

I formerly thought that the substance in question was a compound of 1 equivalent of hydrogen + 1 equivalent of carbon and proposed calling it *protohydruret* or *protohydroguret* of carbon, but we have no proof of its being so constituted: for although 1 equivalent of the oxygen of the water evidently combines with 1 equivalent of carbon to form 1 equivalent of carbonic oxide, it is not evident that the 1 equivalent of the hydrogen of the water also combines with 1 equivalent of carbon; it may combine with 2, 3, or more equivalents.

The production of light under water by means of charcoal connected with a voltaic arrangement is a well known experiment; but I have not met with any one who is aware that the above described change takes place, and therefore I venture to lay these observations before the public through the medium of the London Electrical Society.

When the experiment is made merely to obtain the compound under consideration in what may be considered a *concentrated* state, the less distilled water that is employed the better; about two or three ounces will be sufficient, it being merely requisite to keep the charcoal points covered with it, but more water will be required when it is intended to collect the carbonic oxide.

I have no doubt that some interesting results may be obtained by acting upon other fluids in a manner similar to the above with charcoal; and certain other substances might also be employed to effect the end I have in view; namely, the formation of compounds by producing the electric spark in water and other fluids, by causing the electrodes of a battery to be armed with different materials of a certain kind.

When steam is decomposed by passing it over red hot charcoal for the purpose of obtaining carbonic oxide, I do not find

in the water over which the gas is collected anything like the compound in question. It may not be irrelevant to observe that Mr. Charles Cowper suggested to me that he thought carbonic oxide might be substituted for hydrogen as a combustible with oxygen for obtaining intense heat. A considerable quantity was accordingly burnt with oxygen by means of my blowpipe, and when the jet of inflamed gas was thrown on a piece of lime, the light produced was very little inferior to that which is obtained when hydrogen and oxygen are employed for the same purpose, and probably the light would have been improved had the carbonic oxide been passed through lime to free it from the carbonic acid which it always contains.

May not the hydro-carbon which is the subject of this paper be identical with the oily compound noticed by Berzelius as being produced when hydrogen gas is obtained from iron and dilute sulphuric acid? (See *Lehrbuch*, 186.)

I remain, Sir,

Your's truly,

WILLIAM MAUGHAM.

*Royal Gallery of Practical Science,*
*Adelaide Street, West Strand,*
*June* 21, 1838.

Note. At the ordinary meeting of the Society, July 17, the author of the preceding paper went through the experiments therein detailed, and the several results were highly satisfactory to the members present, being perfectly corroborative of what is stated respecting the formation of carbonic oxide and a previously unnoticed *hydro-carbon* when water is decomposed in the manner described.

---

LXI. *An account of a series of daily observations, made by* Andrew Crosse, Esq., *of Broomfield, near Taunton, with a Sustaining Voltaic Battery, to ascertain the increase or diminution of the power of the same, as corresponding with the increase or diminution of the temperature of the atmosphere, during a part of the last winter, and commenced previously to the very severe frost which afterwards took place. Also a few remarks on the agency of heat in electro-crystallization.*

Read June 19th, 1838.*

For upwards of two years past I have found it convenient in the formation of crystalline and other matters, by the elec-

* From the Transactions of the London Electrical Society.

tric agency, to make use of porous earthen pots, of the same nature as garden pots; but without an aperture in the bottom. One of these being filled with a compound fluid, A and B, and being plunged in a basin filled with another compound fluid, C and D; A of the one fluid having a greater chemical affinity, (as it is called) to C of the other fluid, than it has to B, with which it is united, and also B to D, so that by the admixture of the two fluids, double decomposition would take place; an electric current being passed by means of primary conductors proceeding from the poles of a voltaic battery in constant action, from one fluid to the other, through the pores of the pot employed, a slow union of A and C, or B and D, or both, takes place, either at the positive or negative pole, or on the inside or outside, or *within the substance* of the pot itself; or in more than one, or in all of these, according to the *nature* and *temperature* of the fluids employed, the *intensity* or *quantity* of the electric current, the thickness of the pots, and the presence or *absence of light*, which last is in most cases of greater or less importance, and in some absolutely essential. The result of this union is common to the production of regularly or irregularly formed crystalline matters more or less firmly adhering to the substance upon which or within which they are formed. I have used these pots in hundreds of experiments, in an infinite variety of applications, and with considerable success. I have likewise used them more or less extensively in the place of bladder in sustaining voltaic batteries, for which purpose they are admirably suited. They have, however, one defect. If, while sulphate of copper is used for the negative cells, a neutral salt be employed for the positive, in the course of time crystallizations are formed within the substances of the earthenware which separates the two fluids, and the pots are cracked in all sorts of forms—sometimes longitudinally; sometimes laterally; sometimes in concentric layers, the outer or inner portions scaling off like the bark of a tree; and sometimes in small angular or circular fragments which start off with a slight explosion, so that after some months' action the earthen vessel is spotted over with deep indentions either external or internal, or both. It is therefore safer and better on all accounts to avoid the use of neutral salts in the positive cells, which I commonly fill with simple water, when I wish to keep up a uniform action for a considerable time, and when I employ these pots merely in the place of bladder.

The following observations were made in a room exposed to the light, with a southern aspect, and situated about 800 feet above the level of the sea:—

Dec. 22, 1837. 10 P.M. I set in action a small sustaining battery composed of twelve two-inch square arcs of zinc and copper (the zinc not amalgamated), in small porous pots and glass basins; each zinc plate resting on a small piece of zinc, placed in a glass basin filled with common water; and each copper plate resting on a larger piece of copper placed within a pot which stood in the middle of the next glass basin, and which contained three ounces of sulphate of copper and water. It is a simple and economical mode of increasing the surface of the metals employed, to cause the pairs of plates to rest respectively on larger masses of the same metals, by which means small plates may be made to act in some degree with the power of large ones, and which partially saves the expense and trouble of casting. A Faraday's voltameter being filled with common pond or river water, was connected with the poles of the battery, and emptied of its gas each night at ten o'clock, and replaced in its former situation. A thermometer was likewise suspended above the battery, so that its bulb was immersed in one of the glass basins of water. This thermometer was examined at different intervals during the day and night, and the degrees of temperature carefully noted. It is obvious that by this arrangement only a portion of the electric fluid excited could possibly pass through the voltameter, as no acid was added to the water with which it was filled. For this I had reasons which I shall not here dwell upon.

The observations were continued for the space of one month or twenty-eight days, during which time neither water nor sulphate of copper was added to the cells of the battery, in consequence of which a good deal of the fluid had evaporated at the month's end. When the voltameter was first applied, a very small stream of the combined gases was extricated, but in the course of some hours it increased, and at the end of twenty-four hours 42·20ths or degrees of gas were evolved. It will be seen by inspecting the journal attached, that the battery did not arrive at its maximum of power till the third day from the commencement of its action. This was occasioned by the resistance of the earthen pot to the electric current, a thicker or a thinner pot affording a greater or a less resistance. It will likewise be seen that in general there was a more or less regular decrease of power, which seemed to be at the rate, as near as one may judge, of from one to two degrees of gas in twenty-four hours, supposing the temperature to remain the same; but that in general the power increased or diminished with the increase or diminution of temperature. Thus in the first week, as long as the thermometer stood at about 50, the diminution of gas was from one to two degrees

in each day: but on the last day of the year when the thermometer had sunk from 50.5 to 47, the quantity of gas obtained was lessened from 61 to 57.5 degrees: also between the 6th and 7th of January, with a diminution of temperature of from 42.5 of the thermometer to 39, there was a diminution of gas of from 51 to 45 degrees. This is what one might more or less have expected; but I know not how an increase, and a somewhat considerable one, of the power of the battery could take place under a *diminution* of temperature. Thus, on January 13th, with the thermometer at 32, forty-five degrees of gas were produced; when on the preceding day, with the thermometer at 36, only 42 degrees of gas were liberated. Again, on the 17th of January 47 degrees of gas were produced, with the thermometer at 34, when on the preceding day there were only 41 degrees of gas with the thermometer at 33. The one degree's increase of heat bears no proportion to the six degrees' increase of gas. Again, on the last day of the journal, with the thermometer at 32, there were 4 degrees of gas less than the preceding day with the thermometer at 31. This requires sifting and close examination. It may be observed that the degrees of gas produced on the last day, with the the thermometer at 32, and with ice in all the cells, were exactly the same as on the first day with the thermometer at 50. It may also be noted that the total quantity of gas obtained in the fourth week was only 4.5 degrees less than that which was obtained in the third week, notwithstanding the natural diminution of power in the battery, the increased loss by evaporation of fluids, and the five degrees' diminution of temperatures. I was prevented from prolonging these observations by the freezing of the water in all the cells. I may here observe that I had, previously to these experiments, as I have since tried the effects of heat in combination with voltaic electricity in the formation of crystals, that I have exposed various solutions under different conditions to the electric action, such solutions having been kept as nearly as possible at the boiling point from one to six weeks, the apparatus being plunged in sand baths, with fires kept up day and night without a moment's intermission, and the solutions being constantly replaced as they evaporated. In sixteen of these experiments which were carried on at the same time, the evaporation was so great that it exceeded seven gallons in every twenty-four hours. I am not prepared at present to give a succinct account of the different results of these operations, but shall state generally the following conclusions.

1. A piece of yellow sulphuret of copper was exposed to the electric action, in sulphate of copper at the negative pole,

in the cold solution, and found after a given time to gain a certain weight, the same being my friend Mr. Fox's experiment.

2. A similar piece of the same was exposed, exactly under the same circumstances, *in the hot solution* to the same electric power, and found to gain *thirty-one times* the weight of the preceding, within the same time. Such additional weight in both cases mostly consisted of metallic crystallized copper and red oxide of copper on the surface of each.

3. Although the solutions, in which the latter formation took place, were kept as constantly as possible at the boiling temperature, the crystals were generally of the most regular form, with their angles and facets quite as perfect as those of a natural formation.

4. In the production of crystallized copper and red oxide of copper, I found that with a single pair of plates plunged in boiling solutions the increase of crystallized matter averaged 60 *grains in each day*, or one ounce troy in every eight days; and that, consequently, even tons weight of crystallized copper and red oxide of copper may be formed in a comparatively very short space of time by an increase of electric power, and the quantity of solutions employed. The sizes of my plates were various, generally about two inches square.

5. By covering large plates of zinc, with plaster of Paris, unconnected with any copper plates, and laying them horizontally in large vessels filled with sulphate of copper, kept boiling, and well supplied with a fresh solution as the evaporation went on, the most perfect octohedral crystals of metallic copper and red oxide of copper, were formed to the amount of some ounces in weight in less than ten days. These crystals were fully equal in all respects to those formed by nature.

6. The same effects took place in the cold, but in an infinitely inferior degree. In this way I have formed crystallized copper, silver, and lead, upon a zinc ingot.

7. In breaking the thick earthen pans, in which some of these formations have taken place, crystals of various sorts are found within their substance. Also veins of metallic copper crossing them in various directions, very similar to what are termed the leaders to a metallic lode. Under some circumstances, perfectly insulated crystals of various sorts, not in connexion with either pole or with any metallic substance, are formed in abundance. This I have frequently observed, but in much less quantity, to have taken place in the cold, during the last two years. The above is a correct but rude sketch of some of the general results which I have met with in the combination of heat and electricity.

*Query.* May it not be possible to apply the combined action of a boiling heat and continued electricity, to the extraction of metals from their ores in a pure state, and with less trouble and expense than the plans now adopted?

ANDREW CROSSE.

*London,*
*June* 18*th*, 1838.

*Extract from the Journal referred to in the preceding paper.*

| Day of the week. | Date. | Of gas Degrees. | Therm. | Day of the week | Date. | Of gas. Degrees. | Therm. |
|---|---|---|---|---|---|---|---|
| | 1837. | | | | 1838. | | |
| Saturday | Dec. 23 | 42 | 50 | Friday | Jan. 5 | 52 | 44 |
| Sunday | 24 | 64 | 50 | Saturday | 6 | 51 | 42½ |
| Monday | 25 | 69 | 50 | Sunday | 7 | 45 | 39 |
| Tuesday | 26 | 67½ | 50 | Monday | 8 | 43 | 37 |
| Wednesday | 27 | 65½ | 50 | Tuesday | 9 | 41 | 34 |
| Thursday | 28 | 64 | 50 | Wednesday | 10 | 44½ | 35 |
| Friday | 29 | 62 | 51 | Thursday | 11 | 44 | 34 |
| Saturday | 30 | 61 | 50½ | Friday | 12 | 42 | 36 |
| Sunday | 31 | 57½ | 47 | Saturday | 13 | 45 | 32 |
| | | | | Sunday | 14 | 42 | 30½ |
| | 1838. | | | Monday | 15 | 43 | 33 |
| Monday | Jan. 1 | 56 | 47½ | Tuesday | 16 | 41 | 33 |
| Tuesday | 2 | 55½ | 46 | Wednesday | 17 | 47 | 34 |
| Wednesday | 3 | 54 | 44 | Thursday | 18 | 46 | 31 |
| Thursday | 4 | 52 | 42 | Friday | 19 | 42 | 32 |

| *Gas obtained.* | | *Average temperature.* | |
|---|---|---|---|
| 1st week . . . . | 434° | 1st week | a little above 50° |
| 2d do. . . . . | 388° | 2d do. | not quite 46° |
| 3d do. . . . . | 310½° | 3d do. | not quite 37° |
| 4th do. . . . . | 306° | 4th do. | a little above 32° |

## LXII. *Researches in Magnetic Electricity and new Magnetic Electrical Instruments. By* Charles G. Page, M. D.*

1. *Compound Electro-Magnets for the magnetic electrical spark, shock and decomposition.* In the last number of this Journal, I announced this new form of magnet, as decidedly superior to the common solid electro-magnet, for exhibiting magnetic electrical phenomena. I have since performed a variety of experiments, with reference to the best mode of constructing these magnets, and arrived at some beautiful results and important conclusions. I find that the

* From Silliman's American Journal.

magnet made of flat plates of thin hoop iron, answers best for lifting power and sparks; but those made of fine annealed iron wire, answer nearly as well, and, (for reasons, by and by to be mentioned) will in some cases be found preferable to the flat bars. Sixteen magnets were made of soft iron wire, in bundles of various lengths and diameters. Thirteen were made of No. 26 wire, two of No. 16, and one of No. 8. For lengths within ten inches and diameters within one inch, the very fine wires answer best. But for longer and larger magnets than these, the larger iron wires answer equally well. Each of these magnets were tried with one, two, and three coils of No. 16, copper wire (wound each the whole length of the bars and superposed by a single pair of lead* and zinc plates, eight inches single surface of zinc), kept constantly in good action by the sulphate of copper. One of these magnets weighing two ounces, with four coils of fine wire exterior to three of large, gave a shock which could not be endured. The two best magnets of the sixteen, were, one made of one hundred fine wires six inches long, and one of five hundred wires, ten inches long. This last furnished the most splendid deflagrations, and by far the strongest shocks. Its decomposing power was very great, for the means used. One was prepared of one thousand wires, and a foot long, but its power was inferior to that of five hundred wires. This large compound bar was ultimately sawed in halves, and each half indicated as much power as the whole. This fact seems to prove that the length of the coils of copper wire was too great for the size. I apprehend that a copper wire of a larger size† would have made it superior to the other magnets. The advantage before alluded to, possessed by the magnets prepared from bundles of fiine wire, is, the facility of making a curved or U magnet‡ wound accurately throughout its length. There is little or no advantage in winding common magnets on the

* If properly prepared, lead for the negative plate, is much superior to copper, where the sulphate of copper is used.

† I have universally found that large wire answers best for large magnets, and small wire for small magnets. The larger the wire, the more freely it conducts, but large wire cannot be used with advantage on small magnets, as the coils or turns will not be sufficient in number, and the axis of the wire will lie more oblique to the axis of the magnet, than that of a smaller wire, or than upon a larger magnet.

‡ The term, horse shoe, applied to magnets is inappropriate, and has led many into the error of constructing magnets of this awkward and disadvantageous form. The letter U would briefly designate this species of magnet.

curved portion, owing to the difficulty of laying the wire on that part, at right angles to the axis of the bar. A very perfect U magnet covered accurately throughout its length, is readily made, by winding a straight bundle of fine iron wire, with as many coils of copper wire as desired, and bending them afterward. They are bent with ease, and the wire disposes itself on the bent portion in a beautiful and regular manner. All the magnets that were made of this description, were tried before and after they were bent, and no change in their properties could be observed. The best of the magnets, above alluded to, was covered with two hundred feet of fine wire, exterior to four coils of large wire. Combining the secondary currents of the large and small wire, the shock was so great as to render it difficult to keep even the tips of the fingers upon the wires. A single thermo-electric pair, heated and cooled, connected with the large wire, gave a bright spark and a shock, which could be felt as far as the wrist. The use of three pairs in sequence, enhanced the results. It must here be observed, that the shocks and sparks are not thermo-electric, but magneto-electric. The pure thermo-electric spark, I apprehend, has never yet been seen. I obtained, a long time since, sparks and shocks from a thermo-electric pair, in connexion with Henry's flat spiral. But in that case also, the results were purely secondary, or magneto-electric, the copper spiral while transmitting a current, in fact, representing a magnet with axial poles. I have always maintained the position, that a shock direct cannot be obtained from a single pair of plates, or any elementary current, under any arrangement whatever. Although recently, we have the high authority of Faraday, that iodide of potassium, and some other compounds may be decomposed, by a single galvanic pair, yet even admitting this to be fully established, I see no reason for retracting, and may continue safely to assert, that an elementary battery however large, cannot afford a direct appreciable shock. In Faraday's experiment, the decomposition was the result of uninterrupted action, but in all experiments hitherto, where shocks have been obtained by the aid of a single pair, they have been obtained as single impulses, immediately consequent to the completion of the circuit, as with the large magnet of Prof. Callan, or as in all other cases, to the interruption of the circuit. In a new instrument, shortly to be described, I have a singular instance of an electro-magnet, affording shocks, not only on the completion and breaking of the circuit, but even while the battery current is passing without interruption. It appears irrational to suppose for a moment, that the shock obtained by breaking the circuit with

coiled conductors, can receive any augmentation from the conjunction of the primitive and secondary currents, for the presumption is, and the fact itself seems sufficiently obvious, that the sparks and shocks indicating a new and secondary current are directly consequences of the dissolution of the primitive current. In other words, the effects produced by the breaking of a primitive elementary current, are due solely to magnetic excitation, and have no connexion with that primitive, except that of cause and effect. In fact, strictly speaking, the results thus observed are not secondary, but tertiary phenomena; the secondary production being the development or neutralization of magnetic forces. And, as Mr. Sturgeon has very ably set forth in his beautiful theory of electro-magnetic lines, in the present state of our knowledge, it is indispensable to the explication of the reciprocal action of magnetism and electricity, to *suppose* the existence of a secondary intervening medium, whether the coiled conductors act with or without the co-operation of ferruginous bodies. One singular fact which I noticed, nearly two years since, remains to be reconciled with the postulates, that a direct shock cannot be appreciated from an elementary current, and that secondaries are strictly consequences remote from primitives. When two, three, or four pairs of plates, or any number below twelve, arranged as a compound series, are connected with coiled conductors, with or without soft iron enclosed, the sparks and deflagrations, are far more brilliant on breaking the circuit, than with the same plates used as a simple or elementary battery. This fact is readily accounted for. The battery current itself is capable of passing through perceptible space, with appreciable duration, and as the secondary current (which is the natural electricity of the wire, set in motion by magnetic forces), returns to its equilibrium through the medium of the battery plates and its liquid, the two currents must here move in conjunction, and enhance the combustion of the metals used. I have strictly examined the power of the pure secondary developed in this way, and find it never to exceed the secondary from the same pairs arranged as an elementary series. In fact, the secondary begins to diminish, just when the magnetizing power of the compound battery begins to diminish, which is well known to occur, as the series extends. The properties of small and large compound batteries, have moreover been examined while the currents have been passing through circuits from twenty feet to half a mile, without any observable characteristic changes. But to return to the action of the compound electro-magnet. The superior value of these magnets,

whether made of flat plates or fine wire, may be traced to several causes. First, the homogeneous texture of the iron from which wires or flat plates are made, is favourable to the extensive development of magnetism. Secondly, the same quality favours its neutralization, when the exciting cause is withdrawn. Thirdly, the sum of the actions of so many highly charged magnets, and lastly, the mutual neutralizing influence of the individual magnets of a similar polar arrangement. This last is by far the most important consideration. The following experiments, throwing light on this point, will be regarded as novel, and have an important bearing on the subject of magnetism itself.

Take a piece of fine iron wire, carefully annealed, three or four inches in length, and touch it once with a common steel magnet. Being very soft, it yields readily to the inductive influence, and as its length is very great, compared with its breadth, if carefully handled, it will be found to retain sufficient power to hold more than its own weight. Hold it now by one end and snap the other with thumb and finger, and its magnetism will be instantly lost. The same quality of the iron which favoured the extensive development of magnetism, favoured also its neutralization on the disturbance of molecular forces. The transition here was so sudden and decided, that I was led to try the influence of a magnet thus operated upon, in developing electricity. A long piece of iron wire, retaining considerable magnetic power, was suspended so as to vibrate freely in a spiral of copper wire. A smart rap upon the suspended iron wire, determined a strong galvanic current through the copper spiral, as indicated by the galvanometer. The result, though readily anticipated, was nevertheless very striking. Again, take a number of pieces of fine iron wire, and magnetize them separately, so that each by itself will hold its own weight: combine them now in a bundle, and instead of the aggregate lifting power of the elements, you scarcely realize the power of one of those elements, owing to the neutralizing influence of the similar poles. It is the co-operating, neutralizing effort of similar poles, that chiefly determines the superior value of the compound electro-magnet for magnetic electrical experiments. The fact that very fine steel wires answer almost as well for these magnets as wires of soft iron, strongly corroborates this last position. For the knowledge of this curious fact I am indebted to Mr. Daniel Davies, philosophical instrument maker, of Boston. A bundle of fine steel wires when wound, give a bright spark and strong shock, whereas the same amount of steel in a solid bar, produces a hardly perceptible augmentation.

2. *New Magnetic Electrical Machine, or Magneto-Electric Multiplier, convertible into an Electro-Magnetic Engine.* In order that this machine may be readily comprehended, it will be necessary to make a few brief preliminary observations, and to advert to the discoveries which led to its invention. When a piece of soft iron enclosed in a helix of wire, is rendered a magnet, by the approach of a magnetized bar, a current of electricity flows through the helix (during the *development* of magnetism) in a direction contrary to that of a galvanic current, which would render the piece of soft iron a magnet of the same character. Withdraw the inducing magnet, and during its withdrawal, or the neutralizing process in the piece of soft iron, the direction of the electric current in the helix will be the *same* as that of a galvanic current which would render the soft iron a magnet of similar polar arrangement. Similar momentary currents are excited when the exciting cause is a galvanic current. *Completing* the galvanic circuit with the helix, determines a current flowing against the battery current. *Breaking the circuit*, determines a current flowing in the same direction with the battery current, which produces the bright spark. Any disturbance of magnetic forces is accompanied with a disturbance of electric equilibrium. It is well known, that an electro-magnet possessing an immense lifting power when its poles are joined by an armature, exerts a comparatively feeble action at a distance. The reason is this; the two magnetic forces or poles are tending constantly to neutralize or disguise each other, the softness of the iron favours this mutual action, and the whole amount of magnetism developed cannot be perceived, until the magnetic forces are insulated or determined towards the poles by the application of an armature. The application then of the armature must occasion a considerable disturbance or movement of magnetic forces, and give rise to an electrical current in the wires. The direction of this current can easily be predicted, from what has been before said. The application of the armature is equivalent to the further development or separation of magnetic forces: hence the new current will flow against the battery current. When the armature is pulled off, the new current is in the same direction as that of the battery. And if the magnet wires be so arranged that the galvanic circuit may be broken at the instant of pulling off the armature, it will be found that the magneto-electric spark and shock will be far greater, than can be obtained by simply breaking the circuit, without the aid of this operation. Thus we have a new method of augmenting to a great degree the magneto-electric currents

from an electro-magnet. The application of the armature contributes to the development of magnetism, and on the simultaneous withdrawal of the armature, and the galvanic current, the whole amount is suddenly neutralized.* The current developed by the *application* of the armature, I have appreciated by the following experiments, and I find its consideration to be of very great importance in the application of electro-magnetism as a moving power. This subject will be considered at some future time, and at present I shall be content with giving briefly the reasons why we cannot increase the power with economy, where a great uumber of magnets are charged by the same battery. 1. The motion of attracting poles towards each other, actually diminishes the power of each magnet, by determining a current against the current of the battery. 2. The succession of similar or repelling poles, determines a current also against that of the battery. 3. The withdrawal of attracting poles (which must be effected by mechanical power, and is of course directly against an independent movement) maintains the power of the magnets, as the new current is then in favour of the battery current.

*Exp.*—To test the value of these secondary currents, I included a galvanoscope in the circuit with an electro-magnet, and placed the needle beyond the direct influence of the magnet. On bringing up the armature, the needle returned four degrees, and on suddenly applying it, the needle swung nearly back, but immediately returned to nearly its original deflection of 40°. Suddenly pulling off the armature, the needle swung over 90°. The following experiment is still more striking. Connect two electro-magnets, so that they shall be charged in sequence, but at the same time, and with the same battery. Load one of them with about as much as it will hold. Apply the armature to the other magnet, and the weight of the first magnet will immediately drop. The same may be repeated with either magnet. The power of this reacting current, as it may be called, is in proportion to the number of magnets in use, charged by the same battery, the number of coils of wire covering the magnets, and lastly in proportion to the rapidity of motion.

The operation of the magneto-electric multiplier will now be understood at a glance. It is simply an electro-magnet,

* A steel magnet gains power slowly when its poles are armed and loaded simply by determination, or insulation of its poles. Pulling off the armature again weakens its power. Jerking it suddenly off, weakens it still further. In this case the magnetic forces seem to acquire momentum, and go beyond their original statical development.

with a revolving armature and break-piece. The revolution of the armature is of course equivalent in its effect, or nearly so, to its rectilinear approach and abduction. *a* fig. 2, plate XIII. is a small compound electro-magnet, 12 inches in length before bent, composed of 500 fine iron wires, covered with 5 superposed coils of large copper wire, No. 16, and 7 layers of fine copper wire, No. 26. *b, c,* are the terminations of the small wire, *d, l,* the terminations of the large wires. 1, is a wooden strap, which, with a binding screw, secures the magnet to the wooden block *k.* *s* is a brass strap, to hold firmly the poles of the magnet. *e* is the armature of soft iron, which, with the small brass pulley, is fitted firmly to the shaft. *o,* the multiplying wheel. *n,* is the break-piece, which is merely a copper ferule dissected, and filled up with wood or ivory, as represented by the shaded spaces. *l,* one of the magnet wires bent up, plays upon the whole portion of the ferule: and *m,* one of the battery wires, plays upon the break-piece. The other battery wire is to be connected with the mercury cup *d.* Adjust now the break-piece, *n,* or the copper wire, *m,* so that the circuit shall be broken at the time or immediately after the armature leaves the magnet, and turn the multiplying wheel *o.* The sparks and shocks thus produced, will be found far greater than when the circuit is broken while the armature is approaching. The break-piece is readily adjusted, so that the armature revolves of itself with great rapidity; but in this case also, the sparks and shocks* are at once diminished, as the source of magnetism must be cut off before the armature arrives at its equilibrium. With things thus arranged, connect the two ends of the small wire *b, c.* (This wire is insulated, and entirely independent of the large wire.) The revolution of the armature is now suddenly stopped. The flowing of the secondary current after the galvanic is broken, keeps up or prolongs the power of the magnet so as to retain the armature. As a proof of this, set the spring and break-piece so that the magnetic power shall be cut off, some time before the armature arrives at equilibrium. It revolves of course slowly. Connect now the ends of the small wire *b, c,* and it immediately revolves with rapidity, the prolongation of magnetism by the secondary, contributing now to the motion of the bar. Here then the secondary becomes a new source of magnetic power. This instrument, when the armature is revolved mechanically, affords a shock from *b, c,* both when the circuit is completed and broken. The sparks are exceedingly brilliant, and the shocks so powerful that

* The shocks are taken from the cups *b, c.*

they are sometimes felt by the bystanders through the floor. The light from charcoal points is intense. It decomposes pure water, and charges the Leyden jar. But its most remarkable and novel property is that of giving a slight shock without breaking the circuit, and while the galvanic current is flowing without obstruction. To effect this, place the spring-wire *m*, with *l*, on the whole portion of the ferule and turn the wheel. A slight shock will be felt at *b*, *c*, being simply the consequence of magnetic disturbance by the motion of the armature. A single thermo-electric pair of bismuth and antimony plates heated and cooled, affords a shock which is felt as far as the wrist.

*Washington, D.C.,*
*June* 2, 1838.

---

LXIII. *On an electro-magnetic Indicator. By* J. T. Quekett, Esq. *In a letter to the Editor of the Annals of Electricity.*

Sir,

Knowing that considerable difficulties are experienced by lecturers and others in the explaining to the uninitiated the action of electric currents on the magnetic needle, I have contrived a little piece of apparatus, which I call my electro-magnetic indicator, for the purpose of illustrating the suggestions of Ampère, and which I think will not only assist in the comprehension of his views, but tend to fix more permanently in the mind this important principle. The construction of it is as follows:—

Let A B represent a piece of wood about eight inches long and four wide, into which is morticed, at a convenient distance from one end, an upright, C D, five or six inches in height, and of the shape represented in fig. 3, plate XIII; on the inner face of this upright are fastened two concentric circles of brass, E F, also seen in fig. 4, and in section in fig. 4. These circles are about a quarter of an inch wide, and the larger is three inches, and the smaller two inches in diameter. To the larger or outermost of these circles is fastened a wire, P, for communication with the positive pole of a battery, and to the inner another wire, N, for the negative pole of the same battery. W W′ is a piece of brass wire bent as represented, and its ends kept apart by a piece of ivory, I I′, through the middle of which is a hole for receiving a screw, M, fig. 3. The end, W, of the wire is made quite smooth and moves on the outer circle, E, whilst the end, W′, moves on the inner

circle, F. These ends are kept in perfect contact with the circles, E F, by means of a spring, G G′, which is interposed between the head of the screw, M, and the ivory, I I′. The screw also forms an axis on which the frame, W W′, is made to turn; the pressure to ensure perfect contact being made by the nut, L, as seen in fig. 3. From the head of the screw, M, we have a smaller wire proceeding, and bent three times at right angles as seen at H, its free end being pointed for the purpose of allowing the magnetic needle, N S, to move on; which needle, for the convenience of persons at a distance, has on its respective poles the cardboard letters N S. Attached to the wire frame at W, is a little wooden figure with its face fronting the needle, and in its right hand is placed a little staff to which another letter N is attached. On the wires, P and N, are fastened arrows, also cut out in cardboard, to show at a distance through which wire, and in what direction, the current is passing; the letters N and P being also used to denote which electricity is employed.

Such is the description of the apparatus. Now for its application.

We will suppose that the wire, P, is in communication with the positive pole of a battery, and N with the negative pole; the arrows then will show that a positive current is passing from the head of the figure towards its feet over the needle, N S; we shall then find that the north pole of the needle, on which the letter N is placed, will move towards the right hand of the figure. But the wire frame, W W′, has a motion round M as a centre, and can be placed in any position, with respect to the needle, N S, that we may please: the contact with the circles, E F, being always preserved by the spring, G G′. If now the frame work be turned so that the figure be exactly under the needle, the north pole will still move towards the right hand of the figure, and it will be observed that the right hand is now in an opposite direction to what it was at first. Again, if the wire be turned a quarter of a circle either backwards or forwards, so that the plane of the wires corresponds with the plane of the needle, the north pole, N, will then dip up or down according to whether the right hand of the figure point upwards or downwards; in fact, as long as the current remains the same, the north pole of the needle will always move to the right hand of the figure, in whatever position the frame, W W′, be put. If a negative current be passing through the wire, P, the reverse law obtains. All these conditions with one current may be shown without removing the wires, P N, from the battery; but the motions

of the needle will be more striking if they be taken out previous to the shifting of the frame work, W W′.

Should you think the above worthy of a place in your Annals, the insertion of it will oblige,

Your obedient servant,

*50, Wellclose Square.* J. T. QUEKETT.

P. S. Since the above was written I have made an improvement in the circles of brass, E F. Instead of having on the outer one a continuous metallic surface, I propose to have at the four quadrants little pieces of brass soldered on, and the space between these pieces of brass to be filled up with wood, so that when the wire touches the wood the current may be stopped, and, consequently, when the wire frame, W W′, is being moved on the wood, the needle will return to its original condition, and only be again acted on when the current is allowed to pass by means of the piece of brass at the quadrant; this contrivance will supersede the necessity of removing the battery wires when we wish to show the action of all the currents in succession. This mode of making contact will probably be of some service to those engaged in telegraphic and other applications of electricity.

## LXIV. *Preliminary observations on an experiment on the action of Perchloride of Platinum (Platinchlorid) on Acetone. By* WILLIAM C. ZEISE, *of Copenhagen.**

When a solution of perchloride of platinum in about two and a half parts of acetone is distilled to a syrupy consistence, and the distillation of the product is once or twice repeated, we obtain a mixture of a variety of new combinations in the several chlorides thus produced. The liquor which last distils over is rich in muriatic acid, and contains at least *one* ætherial body. The different compounds thus mixed together, are difficult to obtain in a separate and pure state: and although I have already devoted much time to accomplish this object, I am, nevertheless, not certain that I have obtained them all in a pure condition. Of these compounds, there appears one which deserves a particular description. It is a yellow crystallizable body whose elementary composition is

$$Pl+2Cl+6C+10H+O.†$$

* Poggendorff's Annalen der Physik und Chemie. Band XLV Stück 2. Translated by W.

† The original formula is Pt+2Cl+6C+10H+O. TRANS.

In order to obtain this compound, I agitate the brown acid, tar-like residue, with fresh portions of water, as often as that liquid assumes a brown yellowish colour, and then quickly filter the solution through a linen cloth, for the purpose of separating the undissolved resinous or pitch-like matter. The solution soon becomes turbid from the lower part upwards, and in the course of half an hour or an hour, a tolerable large quantity of small yellow crystals form and precipitate in the liquor. I now decant the mother liquor and place it in a vacuum over sulphuric acid, either with lime, or hydrate of potassa, until it has evaporated to a brown crystalline mass. This crystalline substance I now treat with water in the same manner as I treated the tar-like substance in the first instance; by which means I obtain another portion of the new crystallizable compound: but this latter portion is generally of a deep brown colour. In order to obtain it pure, I dissolve it in the acid liquor obtained from the first distillations of the perchloride with acetone, and distil this filtered solution down to a syrupy consistence, and treat this residue with repeated effusions of water in the manner already described. Finally, in order to remove every trace of brown colouring matter, I place the last obtained crystalline product between folds of blotting paper, and after its being well pressed and dried I dissolve it in acetone, filter the saturated hot solution into a wide-mouthed glass vessel; and when cooled, decant the liquid part from the deposited crystals. This liquid I cautiously distil down to that state which will ensure the greatest part of it to crystallize. The crystals, thus obtained, are washed in a small portion of acetone and then dried. I have also obtained a considerable quantity of this compound by placing a given quantity of acetone mixed with perchloride of platinum, in a well closed glass vessel: agitating the mixture, and afterwards permitting it to stand unmolested for twenty-four hours. I name this new compound *Metacechlorplatin.*

Metacechlorplatin is of a sulphur yellow colour; the crystals are small and difficult to describe with accuracy: it is almost devoid of all smell. When dried at the common temperature of the air, it loses nothing in weight by being placed in a vacuum over sulphuric acid; not even when the temperature is raised to above 100°. It inflames with facility, burns with a partially green flame and leaves a residue of platinum of a silvery white colour. Heated in a retort it blackens, and without swelling up, yields an abundance of odourous vapour, peculiar at the commencement, but which afterwards has a strong smell of muriatic acid: a part, at least, of which vapour is easily condensed to an oily substance.

The carbonaceous residue burns slowly in the open air, something like tinder, and leaves silvery white platinum behind. Scarcely any of it is dissolved by water at common temperatures, but when heat is applied, a portion becomes dissolved, and forms a yellow solution; which, however, holds but very little of the salt. By boiling, this solution yields a brown flocculent substance, whilst the undissolved portion becomes transformed into a brown slimy mass, without any observable appearance of metallic platinum. Æther does not appear to dissolve this body: and alcohol, at common temperatures, has but a feeble action on it, though, by heat, some little becomes dissolved in the latter medium, which assumes a yellow colour, and which, whilst cooling, deposits a yellow crystalline powder. Muriatic acid, even when concentrated, operates upon it only at a high temperature: the acid solution endures a boiling heat without any observable change. By a solution of potassa metacechlorplatin is easily dissolved, forming with it a brown liquid. A solution of chloride of potassium, or chloride of sodium, also dissolves it by heating: and the yellow coloured solution denotes no change by submitting it to a boiling temperature.

The determination of the carbon and hydroygen was accomplished by burning a portion of the new compound, with oxide of copper, and another portion with chromate of lead.

The composition of metacechlorplatin

$$(Cl.Cl^2+C^6H^{10}O$$

when compared with that of acetone ($C^6H^{12}O^2$), indicates that ($H^2O$) has become displaced by the introduction of an atom of the chloride of platinum. There appears, however, to be several other chloro-combinations formed simultaneously with this one; and, therefore, the chemical interchanges may, perhaps, be of a somewhat more complicated nature. At all events, it is probable that there is formed a compound consisting of two atoms of chlorine and two atoms of hydrogen, in the same manner as by the action of perchloride of platinum with alcohol; and there is also formed, by the reaction of one atom of oxygen, one of the compounds of a corresponding character with aldehyde (Consult my essay on the inflammable chloride of platinum in this Annals,* Vol. XL. p. 251). This compound assimilates itself also with those products of acetone with sulphuric acid, muriatic acid, &c., described by Kane (See Transactions of the Royal Irish Academy). It also appears worthy of observation, that whilst by the action of perchloride of platinum on alcohol two atoms of chloride

* Poggendorff's Annalen der Physik und Chemie.

combine with one atom of ætherin ($C^4.H^8$); but by allowing only one atom of chloride, still $H^2O$ (and, perhaps, as the representative of the second atom of chloride), with $C^6H^8$ forms a distinct combination.

When the brown crystalline mass, obtained in vacuo, has yielded its last portion of metacechlorplatin, there remains a sour brown liquid behind. If this be heated in a retort, it becomes troubled and exhibits a tolerably brisk effervescence, yielding, at the same time, an oily looking fluid which passes over; and in less than the course of an hour there forms in the, now almost colourless, fluid a great quantity of small flakes of a coal-black matter. Of this substance I shall make no farther remark in this place, than that, when gently heated, it inflames with explosion. For the present I name it *pyracechlorplatin*.

When the original syrupy product, of the distilled solution of the chloride, has yielded the whole of its soluble portion to the water, there remains a considerable quantity of a dark-brown pitchy matter; which for convenience I call *platinharz*. At common temperatures this substance is as hard and brittle as rosin, exhibiting a glassy fracture: and when very carefully cleansed with water, and then dried in a vacuum over sulphuric acid and hydrate of potassa, it becomes very easily pulverized. When something warm, it becomes as soft and pliable as wax, and is easily drawn out into threads. When inflamed, it emits a brilliant flame, which near the border is of a greenish tinge, and leaves metallic platinum. Heated in a retort it swells considerably and emits an abundance of fumes, some part of which very easily condenses; and the carbonaceous residue burns slowly in the air, leaving platinum. A solution of potassa dissolves the resin completely, acetone nearly all of it, and alcohol and æther a sensible part. That portion which does not dissolve in the two last-mentioned liquids, forms a solution with acetone, from which æther separates a dark brown substance, dissolvable only in acetone and solution of potassa. This substance, for the present, I call *chloraceplatin*. That part which is dissolved by alcohol and æther, appears still to contain two distinct bodies. These, as well as the metacechlorplatin, and the remaining primary and secondary products, I hope, soon to be able to give a more copious description. In connexion with this enquiry, I am also preparing for an examination of the action of metaceton, pyroligneous spirit and oil of turpentine with perchloride of platinum. I also think of examining some other metallic chlorides and haloides with reference to those I have already studied.

LXV. *On some electrical experiments. By M. Voort, Director of the Department of Natural Science in Felix Mertis. In a letter addressed to the Editor of the Annals of Electricity, &c.*

Sir,

On reading your memoir on the electric fluid, which appeared in the Annals for June last, I became desirous of making known to you an experiment which I have made two years ago and communicated to our Society, Felix Mertis, to prove my conviction that there is a difference between the force of the positive and negative electric fluids, if two such forces do exist; or that, if we were still to keep close to Dr. Franklin's theory, which I always admired, its passing from one place to another might be made visible. For this purpose I employed the following apparatus:—

Let *a* and *a'*. fig. 5, Plate XIII., be two small Leyden jars of three inches high and without covers, having the two conducting wires of thin brass fastened in the bottom. Charge them at the same moment in the usual way at the conductor of an electrical machine, only to a low degree; then place them on another table at a great distance from the machine to avoid every influence. Put one of these charged jars on a stand of glass *g*, and the other on the table as shown in the figure. Between these jars place a glass pillar, *o*, having a steel point on its top, and on that point an index of 12 inches in length is placed, which may move freely in any direction. This index is better represented in fig. 6, where *c* is the agate cap which rests on the pivot; *c g* a capillary glass tube with a fine copper ring at its outer extremity; and *c c* a thin copper wire with small counterpoise. In commencing the experiment the ring should be held fast either with the finger or, which is better, by a piece of glass, just in the middle between the *outside* of the jar on the glass stand, and the *conducting rod* resting in the inside of the other; so that it has *plus* and *minus* electricity on the opposite sides of it. If there be no current of air nor moving of persons to disturb its tranquillity in the room, conditions highly essential to ensure exact results, you will perceive that, on leaving the index to itself, exactly at right angles to a right line joining the two jars, it will instantly move first to the positive side, (towards the rod of the jar *a*, on the table) where it will take a certain quantity of electricity: enough to cause it by repulsion, from thence, to go over to the negative side of the jar *a*, which is placed

on the glass stand; and in this manner it will play to and fro for a few minutes, causing the charge of the two jars to diminish sensibly. Out of a hundred experiments of this kind, the first movement was eighty times towards the positive surface; and whenever the first motion of the index was towards the negative surface it was not difficult to see that there was a defect in the manner of operating either by a little motion of the hand whilst leaving the index, or from some other cause. I need not observe to you, sir, how difficult it is to proceed with accuracy in such extremely delicate experiments. If the jars are loaded too *strong* the movement of the index will be too swift and uncertain. There must be just enough force, and no more than is wanted, to produce a gentle movement.

With high regards, I am,
Your obedient servant,
A. VOORT.

*Amsterdam,*
*December 25th,* 1838.

## LXVI. *Curious Voltaic Experiment.*

We are indebted to Mr. Porrett for the discovery of a disturbance of the common hydrostatical level of water by voltaic action. Mr. Porrett's interesting experiment is well described in the 8th. volume of the Annals of Philosophy. The arrangement consisted of a glass vessel divided into two compartments by a bladder partition. "One of these compartments having been filled with water, and left for several hours, was found to have retained the water. The bladder, therefore, was not sufficiently porous to allow the water to filtrate through it. The cell filled with water, was now positively electrified, with a (voltaic) battery of 80 pairs of $1\frac{1}{4}$ inch double plates, and a few drops of water were put into the empty cell, so as to cover the bottom of it. This small quantity of water was then negatively electrified" by placing in it the wire from the negative pole of the battery. "Independently of the decomposition of a small part of the water, which of course took place in the usual manner, the principal part of it obeyed the impulse of the voltaic current, *from the positive to the negative wire,* first overcoming the resistance occasioned by the compact texture of the bladder, so as in half an hour to have brought the water in both cells to the same level, and afterwards overcoming the additional resistance occasioned by the gravitation of the water, by continuing to convey that fluid into the negative cell, until its surface in that cell was upwards of $\frac{3}{4}$ of an inch higher than in the positive cell." This experiment has been repeated by M. De la

Rive, and Mr. Mullins, with similar results.* But the former gentleman has stated that he could not succeed unless distilled or river water was placed in the cells, for when he employed a saline solution of a proper strength no such effect of impulsion was perceptible.†

Having ascertained the correctness of the statements of Mr. Porrett, I became desirous of knowing whether or no a feebler voltaic intensity would produce a similar effect by a longer continued action: and as earthen-ware diaphragms are now much employed for separating the copper from the zinc of a voltaic pair, I employed an unglazed stone-ware jar, which was kindly given to me by Mr. Gassiot, for a first trial. This jar is about 6 inches high and $3\frac{1}{2}$ diameter. It was partly filled with a solution of carbonate of soda, and placed in a larger jar of porcelain, which was afterwards filled about half way up with a solution of sulphate of copper. A cylinder of rolled copper, sufficiently large to envelope the diaphragm, was placed in the sulphate of copper, and a small cylinder of zinc in metallic connexion with the former, was placed in the solution of soda. The liquids inside and outside of the stone-ware diaphragm were of precisely the same altitude. In three days a difference of altitude in the two liquid columns was very perceptible, and in about ten days from the commencement of the experiment, nearly the whole of the liquid in the zinc cell was driven through the stone-ware partition into the outer compartment. The copper solution was so completely decomposed, that scarcely a trace could be detected by liquid ammonia: and but a very slight tinge was given to litmus paper, showing that the acid liberated from the sulphate had been either neutralized by the soda, or decomposed by acting on the zinc: or its disappearance was probably occasioned on both these accounts. A thin crust of salt, (probably sulphate of soda) covered the moisture which was left in the bottom of the diaphragm, and the zinc was much corroded and partially covered with a white matter, partly soluble and partly insoluble in water, the latter portion being a carbonate of the oxide of zinc. I have repeated the experiment with precisely the same results, obtaining a difference of altitude of four inches in the two liquids, the filtration being always in the direction of the electric current. I have also obtained similar electro-transportations of liquid media through diaphragms of common tile, and soft red brick, and I have no doubt of their being obtainable, by sandstone and even thick masses of granite.

WILLIAM STURGEON.

* Annals of Electricity, &c., Vol. I. p. 206.
† Ibid.

## LXVII. *Description of a new Galvanic Battery.*

Mr. Grove has made a voltaic battery consisting of alternate plates of iron and unglazed porcelain, fixed in a mill-board box, at some distance from one another, like the plates of a Cruickshank's battery. When the alternate cells are filled with a solution of the sulphate of copper, one side of each iron plate soon becomes coated with copper. The battery now consists of copper and iron, as decidedly as the Cruikshanks consists of copper and zinc; so that when the other alternate cells are filled with any liquid which will not act on the other face of the iron plates, the battery is ready for experiment. This is an exceedingly ingenious contrivance, but can only be used to advantage when solution of copper is one of the liquids employed.

The battery I am about to describe will be found exceedingly useful either for display on the lecture table, or as an implement of research. It consists of a rectangular wooden box as represented at the bottom of Plate IX., in which are cemented a series of voltaic pairs of copper and zinc, in the Cruikshank form. Every cell formed by these metals is divided into two compartments by a diaphragm of mill-board, also cemented into the sides and bottom of the box so as to be completely water-tight. The figure represents six metallic pairs and five diaphragms, the former by the *light* partitions and the latter by the *shaded* ones. As the mill-board is pervious to aqueous liquids it is obvious that any two kinds may be employed which may be found most suitable to operate with, the one on the copper side and the other on the zinc side. Twelve plates, 6 inches square, answer very well for decomposing acidulated water when a solution of sulphate of copper occupies the copper cells, and a dilute solution of soda the zinc cells. Such a battery will be found to possess many recommendations.

WILLIAM STURGEON.

---

## LXVIII. *On Voltaic Decompositions.* *By* G. Mackrell, Esq. *Communicated in a letter addressed to To J. P. Gassiot, Esq.*

Read before the London Electrical Society Jan. 15, 1839.*

Cloudesly Square,
December 27, 1838.

Dear Sir,

The experiments I am about to detail, and the conclusions at which I have arrived from them, having originated from a

* Communicated by the Author.

gases was as follows:—

| | | | |
|---|---|---|---|
| A—2 | experiments, each | 60 | seconds. |
| B—2 | " " | 53 | " |
| C—2 | " " | 57 | " |
| D—2 | " " | 60 | " |
| E—2 | " " | 63 | " |
| F—2 | " " | 52 | " |
| A & B—2 | " " | 32 | " |
| A, B, & C—2 | " " | 24 | " |
| A, B, C, & D—2 | " " | 21 | " |
| A, B, C, D, & E—2 | " " | 19 | " |
| A, B, C, D, E, & F—2 | " " | 17 | " |

It will be perceived

* The solution in the voltameter was one sulphuric acid of commerce and eleven distilled water, by measure.

The view we entertained previous to obtaining these results

was, that
batteries t
quantities
time when
should ha
conds, bu
showing th

It mus
operation,
perfect ag
the first p
a cause of
surface of
it not also
arrangeme
extremity
be a caus
therefore,
metals wi
ployment
tion of the

Believin
gressive d
successive
gations.

The ar
each; the
the negat
prevent ar
might hav
prevent it
before, ar
was, with battery

gases was as follows:—

| | | | |
|---|---|---|---|
| A—3 | experiments, | each | 57 seconds. |
| B—2 | " | " | 58 " |
| C—2 | " | " | 58 " |
| D—2 | " | " | 61 " |
| E—3 | " | " | 190 " |
| F—2 | " | " | 60 " |

It will be perceived there was a considerable defect in battery E; it was therefore removed from the arrangement;—the remaining batteries were again tested, and finding the results as before stated, we proceeded with batteries A, B, C, D, F.

* The solution in the voltameter was one sulphuric acid of commerce and eleven distilled water, by measure.

| | | | | |
|---|---|---|---|---|
| A & F—2 | experiments, | each | 32½ | seconds. |
| B & F—2 | " | " | 35 | " |
| C & F—2 | " | " | 32½ | " |
| D & F—2 | " | " | 32½ | " |
| A, B, & C—2 | " | " | 25 | " |
| A, C, & D—2 | " | " | 25¼ | " |
| A, B, & F—2 | " | " | 25¾ | " |
| A, B, D, & F—3 | " | " | 22 | " |
| A, C, D, & F—2 | " | " | 21 | " |
| A, B, C, D, & F—2 | " | " | 19½ | " |

Having obtained these additional results, we now proceeded to find the ratios of the differences, with a view of ascertaining if they were governed by any regular progression.

It will be found that the mean time required to produce a cubic inch of the gases, in the second set of experiments, was with

| | |
|---|---|
| A single battery | 58.8″ |
| Two batteries | 33.12″ |
| Three " | 25.33″ |
| Four " | 21.5″ |
| Five " | 19.5″ |

but if the batteries were acting separately the results would be

| | |
|---|---|
| For one battery | 58.8″ |
| Two batteries | 29.4″ |
| Three " | 19.6″ |
| Four " | 14.7″ |
| Five " | 11.76″ |

the difference, therefore, arisiring from interference, between the view we had taken and the experiments, will be

| | |
|---|---|
| In two batteries | 3.72″ |
| Three " | 5.73″ |
| Four " | 6.8″ |
| Five " | 7.74″ |

From these ratios, it appears that the following formula will very nearly give the true result. $a$ being taken for the excess of time required for two batteries in union, over the same two batteries when acting separately. In the foregoing case, $a$ is seen to be 3. 72″; then $a+\frac{a}{2}$ will express the excess for three batteries; $a+\frac{a}{2}+\frac{a}{3}$ that for four batteries; $a+\frac{a}{2}+\frac{a}{3}+\frac{a}{4}$ that for five batteries; and so on; thus

$$a+\frac{a}{2}+\frac{a}{3}+\frac{a}{4} \ . \ . \ . \ . \ +\frac{a}{n-1} \text{ for } n \text{ batteries.}$$

The following table will show the result from our experiments.

| | Mean time by experiment. | Mean time of batteries were acting separately. | Difference. | Difference by the proposed formula. |
|---|---|---|---|---|
| First set of experiments. | | | | |
| 1 battery. | 57.5″ | 57.5″ | | |
| 2 .. | 32 | 28.75 | 3.25″ | |
| 3 .. | 24 | 19.17 | 4.83 | 4.87″ |
| 4 .. | 21 | 14.37 | 6.63 | 5.96 |
| 5 .. | 19 | 11.5 | 7.5 | 6.78 |
| 6 .. | 17 | 9 5 | 7.5 | 7.43 |
| Second set of experiments | | | | |
| 1 battery. | 58.8 | 58.8 | | |
| 2 .. | 33.12 | 29.4 | 3.72 | |
| 3 .. | 25.33 | 19.6 | 5.73 | 5.58 |
| 4 .. | 21.5 | 14.7 | 6.8 | 6.82 |
| 5 .. | 19.5 | 11.7 | 7.8 | 7.75 |

The close approach of the proposed formula for the interference to the observations is remarkable, being in the second set of experiments within one seventh, one fiftieth, and one twentieth of a second; and may not even these trifling differences arise from the imperfect nature of the experiments? It is therefore desirable the subject should be investigated by more correct manipulation than can be expected from amateurs; and we trust that some gentlemen more competent to the undertaking will shortly perform this task, particularly as relates to the difference of the two first batteries, from which our standard data of interference are assumed; and the laws of which our time did not allow us to examine.

We are aware that this communication may be objected to, as it adds nothing to physical science; but it must be admitted, that if the interference of voltaic arrangements can be estimated from an examination of the two first batteries, one point at least is gained: and if electricity can be brought under mathematical laws, the benefits emanating from thence would be incalculable.

I remain, dear sir,
Your's very truly,
G. MACKRELL

## LXIX. *Facts and observations for the purpose of illustrating a Theory intended to connect the Operations of Nature upon general principles.* By Paul Cooper, Esq.

*(Continued from page 403.)*

152. Having investigated the nature and characters of the resistance presented by the interposition of a reversed

pair of plates, I shall now endeavour to explain the cause of resistance when plates of platina only are interposed.

When only one pair of zinc and platina plates was used, the current of electricity was entirely stopped to all practical purposes by interposing one platina plate, fig. 60, (Plate XI.) i. e. by requiring of the current that it should decompose water, and evolve both its elem[illegible] it should pass. This consequence is in perfect ac[illegible] th the views before given. For as the whole result [illegible] n the opposition of forces at the places of electric e[illegible] nd electro-decomposition, and as water is the subst[illegible] decomposed at both before the current can move, [illegible] be expected that the zinc should have such power[illegible] n for oxygen, as not only to be able to take it from its associated hydrogen, but leave such a surplus of force as, passing to the second place of decomposition, should be there able to effect a second separation of the elements of water. Such an effect would require that the force of attraction between zinc and oxygen should under the circumstances be *at least* twice as great as the force of attraction between the oxygen and hydrogen. (Researches 1011.)

163. Platina is naturally negative to hydrogen (87), and in this state the electro-motive force would be from the platina to the hydrogen; but the inductive influence of the zinc working upon the end plate through the wire, &c., produces the arrangement in this plate represented in the figure by the usual positive and negative signs: in this state the surface of the platina presented to the hydrogen is positive to it, and the electro-motive force is from the hydrogen to the platina. Now this arrangement is quite as necessary in the interposed platina plate as in the end plate, not only to bring the particles of water into the electrolytic order described in the figure, but also to enable the current to take the proper direction; it being essential to the production of electrolytic effects in the second cell that it should enter this cell from a negative surface of the platina (140). But the inductive forces of the connected zinc and platina plates can only be conveyed to the interposed platina plate through the medium of the electrolyte; and these forces, thus conducted, are unable to control the electro-motive force of the platina to the hydrogen, and give a negative surface to it upon the opposite side of the plate to that which presents it under the inductive influence of the hydrogen. The polar arrangement in the interposed plate, as here represented in the figure, which is essential to electrolytic action, does not, in fact, take place under these circumstances, and the current as far as this principle is concerned is entirely stopped. The latter part of the para-

paragraph quoted (152), clearly shows that Dr. Faraday attributes the electro-motive force to the greater attraction of zinc for oxygen than hydrogen for oxygen (80); but I would with great deference suggest to him, that this conclusion is in direct opposition to his own more general views. (Researches 660.)

154. "When two pair of zinc and platina exciting plates were used, the current was also practically stopped by one interposed platina plate." (Researches 1012.)

"Three pairs of platina plates, fig. 81, were able to produce a current which could pass an interposed platina plate, and effect the electrolyzation of water in cell 4." (Researches 1013.)

In this experiment the electrolytic arrangement described in figs. 80 and 81, was first actually made; the feeble current observed in the other experiments (Researches 1012) must have proceeded from a partial arrangement in cell 1, fig. 80, and a current through the other parts of the circuit conducted upon the same principle as common electricity (110).

155. "The three pairs of platina plates were now opposed by two intervening plates. In this case the current was stopped." (Researches 1014.)

"Four pairs of zinc and platina plates were also neutralized by two interposed platina plates." (Researches 1015.)

"Five pairs of zinc and platina, with two interposed platina plates, gave a feeble current, with decomposition in all the cells." (Researches 1016.)

These experiments may, perhaps, by putting them in a point of view i familiar to us. A little considerat that if A B, fig. 81, be a platina w will form only a continuation of it; wire be presented to the electrolyt be removed without affecting the in this case will be completed by means of the interposed platina plate, through the medium of the electrolyte; but it will be precisely the same thing if we break the wire A B, and instead of completing the c the electrolyte in cell 4, compl the wire B into contact with the electrolyte between its other en from which it was separated. It respect different in principle fro well acquainted; in which we connect a platina wire with the end zinc plate, and a similar wire with the end platina plate, the interposed plate being removed and the opposite ends of

the wires connected through the medium of an electrolyte in the experimental cell. In the experiments with three, four, and five pairs of zinc and platina plates, and two intervening platina plates; the experiments would be the same in principle if we were to remove the interposed plates, break the wire A B at the points A and B, and complete the circuit by interposing, at both these points, an electrolyte similar to that in the cells of the battery. It will be observed, "that variation in the size of the electrodes causes no variation in the chemical action." (Researches 722.)

156. When a zinc plate was substituted for the interposed platina plate in fig. 80, there was as powerful a current, apparently, as if the interposed zinc plate was away." (Researches 1027.)

In this experiment, zinc being positive to hydrogen (86), the electro-motive force of the interposed plate was from the hydrogen to the zinc and from the zinc to the oxygen, in concurrence with the direction of the current; it must, therefore, have had a tendency to increase its force, rather than to obstruct it. When the experiment was varied, by interposing two and three zinc plates, the electric current passed as freely as if there had been no such plates in the way. (Researehes 1029.)

157. "The effects of retardation were altered altogether when changes were made in the *nature of the liquid* used between the plates, either in what may be called the *exciting* or the *retarding* cells. Thus, retaining the exciting force the same, by still using pure dilute sulphuric acid for that purpose, if a little nitric acid were added to the liquid in the *retarding* cells, then the transmission of the current was very much facilitated. For instance, in the experiment with one pair of exciting plates and one intervening plate (152), fig. 80 when a few drops of nitric acid were added to the contents of cell 2, then the current of electricity passed with considerable strength, and the same good effect was produced by the nitric acid when many interposed plates were used." (Researches 1020.)

"When a little nitric acid was put into the exciting cells, then again the circumstances favouring the transmission of the current were strengthened, for the *intensity* of the current itself was increased by the addition. When, therefore, a little nitric acid was added to both the *exciting* and the *retarding* cells, the current of electricity passed with very considerable freedom. (Researches 1022.)

*(To be concluded in our next.)*

## LXX. *The London Electrical Society.*

*Tuesday, January 15th.* A paper on Voltaic Decompositions was read by G. Mackrell Esq. This paper is given entire at page 495.

*Tuesday, February 5th.* A paper was read by C. V. Walker Esq. describing a series of experiments on the decomposing power of a voltaic battery belonging to T. Mason, Esq. The experiments were made at Mr. Mason's house and the battery arrangements were entirely his own. Ninety-nine porcelain jars, each furnished with a copper and zinc cylinder, and fitted up in a similar manner to those used in the previous experiments made at the house of J. P. Gassiot, Esq. and described at page 420 of this volume, were arranged in nine groups in series of eleven jars in each group. The groups were first tried separately in order to ascertain the mean power of each; and afterwards they were combined *laterally*, commencing with *two* groups, and after ascertaining their united power proceeding with *three* groups, and so on, adding another group to every preceding combination, until the whole of the groups were brought into play in eight distinct combinations, each of which had all its *positive* poles in connexion with one of the metals of the gasometer, and all its negative poles in connexion with the other; the *series*, of course, in all cases being eleven pairs. If now we represent the number of groups in each combination by the digits 2, 3, 4, 5, 6, 7, 8, 9, and the quantity of gas liberated in one second of time, by each individual group by $g$, then the gas liberated by each combination of groups would be $2g$, $3g$, $4g$, $5g$, $6g$, $7g$, $8g$, $9g$, respectively, or the quantity of gas liberated in these experiments was exactly proportional to the quantity of metallic surface in the battery, when the time is constant, a result never before verified experimentally. And what was still more interesting, similar results were obtained with two gasometers whose platinum terminals exposed very different extents of surface to the acidulated water in the instruments, only one of those instruments being used in each series of experiments; for although the larger platinum terminals liberated more gas than the small ones, the quantities liberated by each gasometer were proportional to the voltaic surfaces employed. The batteries were brought into play at 3 o'clock in the afternoon of the 26th Dec., 1838, and were kept in action till 3 o'clock the following morning.* The experiments were conducted by Mr. Mason,

* From these results it would appear that a series of *eleven* pairs of this kind is very near the proper unit of battery intensity for this class of decompositions. Edit.

Mr. Gassiot, Mr. Sturgeon, and Mr. Walker (the same party who carried on the experiments with Mr. Gassiot's battery at Clapham). The whole of Mr. Mason's battery was put in series of 100 pairs for the purpose of ascertaining its *striking distance*, but none could be discovered though sought for with great care. Whether the polar wires were furnished with charcoal points or not, the force of the battery would not strike through the thinnest stratum of air, though a flame of considerable length could be kept in play after once the points had been in close contact and removed slowly from one another.

Read also an extract from a letter from W. L. Wharton, Esq., describing some singular effects of a thunder storm.

*Tuesday, February 19th.* Read an extract of a letter from S. Samo, Esq., dated Surinam, December 31, 1838, addressed to Walter Hawkins, Esq. The Gymnotus Electricus is found in the rivers of Surinam, but the writer states that it is very difficult to select the true from the fabulous accounts of the habits of this creature. It is generally found in shallow rivulets of fresh water, having a rocky uneven bottom, and principally in those parts which are shaded by high trees. Mr. Samo describes the manner in which the eels are caught, and states that one of the three which he had waiting for an opportunity to forward to the Electrical Society, had been attacked and destroyed by water rats; which animals he concludes must be insensible to the electric shock of the gymnotus, although horses can be paralyzed by them.

A paper was read by Mr. Pollock, on the occurrence of the striking distance in frictional, but not voltaic electricity.

Mr. Pollock considers one of the most common of the phenomena attendant upon the action of the electrical machine is the production of the spark upon presenting any body, such as the knuckle of the hand, to the conductor; as this occurs when the body is at a certain distance, this has been termed the striking distance; sparks also occur in the phenomena of voltaic electricity, and it has been very generally supposed, that in this respect there existed no difference in the two actions; but recent investigations, made with most minute attention to accuracy, throw considerable doubt upon this conclusion, showing that it has been too hastily made.

It has been shown on bringing the two terminal wires of a voltaic battery so near each other that the distance between them has been so small as to be scarcely appreciable, yet no spark has occurred; this circumstance compels us to admit, that although the transmission of the spark may not be impossible, yet a most marked distinction between these two kinds of electricity is manifest.

The object of Mr. Pollock's paper is to demonstrate that by the changes in the relations between the rubber and cylinder of the electrical machine, and between the metal zinc and its oxide of the voltaic battery, and their respective latent heats while each is in action, must be of an opposite kind, and, therefore, this distinction between common and voltaic electricity might have been predicted.

Mr. Sturgeon exhibited to the Society the results of his repetition of Mr. Fox's experiments on the conversion of copper ore from one kind to another by the influence of voltaic electricity. (See Vol. I. p. 133.)

The copper ore, in this experiment, formed one side of a voltaic pair, and a slip of zinc the other, being attached to each other by copper wire, and placed in a porcelain pot with a partition of clay between them. A solution of sulphate of copper covered the ore, and water with a few drops of sulphuric acid covered the zinc. The action was kept up for five successive months, during which time several new pieces of zinc had to be soldered to the end of the connecting wire, to replace their dissolved predecessors: so that much sulphate of zinc was formed in the zinc cell, the sulphuric acid being supplied from the decomposed sulphate of copper in the other cell; and the liberated copper attached itself to the copper ore and wire which surrounded it. Fresh supplies of bruised crystals of the sulphate were put into the liquid when found necessary.

When the particulars of the process had been described, Mr. Sturgeon broke the galvanized ore into several fragments before those who attended the meeting, and it was examined and compared with another specimen, also broken at the time, but not galvanized. The two specimens exhibited very different fractures, the ungalvanized being of a brilliant yellow, and the galvanized a dull blackish appearance, something resembling the colour of plumbago. The galvanized ore was completely cased with the precipitated copper, and every fracture presented nearly the same dull colour. It is to be understood that both specimens were taken from the same mass of copper ore prior to the experiment. Because of some difference of opinion existing respecting Mr. Fox's experiment, Mr. Sturgeon did not think proper to give any opinion regarding the nature of change which had taken place in the ore on which his experiment had been made. He brought the specimen without knowing whether any change had or had not taken place; and, having broken it in presence of the Society, and submitted it to their inspection, he considered he had done his duty to all parties, and left the chemical analysis of

the result of this exceedingly tedious experiment, to be undertaken by some other person. J. P. Gassiot, Esq., Treasurer to the Society, took two fragments, one of the galvanized and the other of the ungalvanized ore, which he has put into the hands of an eminent chemist for analysis. The clay partition was not laminated.*

## LXXI. MISCELLANEOUS ARTICLES.

*Mr. Harris's Unit Jar* is represented by fig. 7, Plate XIII. It is formed of a small inverted Leyden jar, supported and insulated by a slender glass rod, covered with lac varnish, and fixed into a wooden sole as seen in the figure. The inner coating of the jar is in metallic contact with a brass ball and wire *d*, and *a*, which wire communicates with a prime conductor. Another ball, above the former, is in contact with the outer coating of the jar by means of a metallic frame and sliding wire. The brass ball *g*, and wire *b*, are also in connexion with the outer coating. If the wire *b*, be connected with the ground whilst the machine is in motion, the jar is charged in the usual manner and is discharged between the ball *d*, and the one above it: and in this way a succession of sparks may be produced by *equal quantities* of the electric fluid. The use of this instrument is to charge other jars with known proportions of the electric matter, which is done by placing the wire *b*, in connexion with the inside of the other jar or jars until a certain number of sparks have been discharged between the inside and outside of the unit jar, and so on to the inside of the charging jars on the table.

---

*To the Editor of the Annals of Electricity, &c.*

*Dundanion, Cork, Nov. 5, 1838.*

Sir,

The Annals of Electricity published by you have lately directed my attention to that very interesting subject. Aware that the success of many experiments depends upon the intensity and permanency of magnetism that can be given to

* Having failed, in two instances, in laminating clay by the action of a single pair, I have placed another portion in the circuit of a series of six porcelain cells, with their copper and zinc cylinders; but as I find that the experiment will be exceedingly tedious, and require attention and time which I cannot well spare, I am not certain that I shall continue it. W. S.

steel magnets, I had 10 of equal size from the same bar of cast steel formed and numbered from 1 to 10. No. 1, 2, 3, 4, 5, 6, 7, and 8, I made as hard as possible, 9 and 10 I left as they came from the forge; all were then ground and polished; 1 and 2 I left perfectly hard; 3, 4, 5, and 6, I tempered down to different shades; 7 and 8 I laid across a bar of red hot iron until the blue shade appeared in the centre, they were then removed and allowed to cool gradually and *when cooling* they were touched with a powerful magnet. All were touched with the same magnet, then tried, No. 7 and 8 raised 20 per cent more than those reduced to a cherry red, and 42 per cent more than No. 1 and 2, and No. 7 was somewhat more powerful than No. 8 which I attribute to its having been touched while in a warmer state.

I have the honour to be,
Your most obedient servant,
R. NEWENHAM.

---

The experienced electrician knows well that if he requires a long spark from the prime conductor of a machine he must commence by taking short ones, and gradually lengthen them to their maximum. A corresponding fact is found in the action of a voltaic battery. No appreciable *striking distance* is passed through by a series of 300 pairs, until the polar wires have first been in contact; this done, however, the fluid may be made to pass through a considerable space by withdrawing the wires gradually from one another. In both cases the short sparks prepare the way for the longer ones, probably by an attenuation of the air, or by a stream of infinitely small metallic particles which are carried between the wires: probably on both these accounts. If the polar wires of a voltaic battery be placed at a small distance from one another, and a Leyden jar be discharged through the intervening space, the voltaic discharge also takes place, and continues as if the wires had previously touched one another. This last fact we believe is due to Sir John Herschel, who produced it a few days ago. EDIT.

---

I have just learned that Professor Jacobi is occupying himself with a discovery which may, if in the end successful, prove of far more use than Daguerre's. He observed that the copper deposited by galvanic action on his plates of copper could, by certain precautions, be removed from those plates in

perfect sheets which presented in relief most accurately every accidental indentation on the original plate. Following up this remark, he employed an engraved copper plate for his battery, caused the deposit to be formed on it, removed it by some means or other, he found that the engraving was printed thereon in relief (like a wood cut), and sharp enough to print from. Whether a repetition of the process from this galvanically formed block will furnish, in its turn, a copper plate from which impressions can be thrown off is not yet established."

JULIAN GUGGSWORTH.

Wormwood Scrubs,
Feb. 5, 1839.

*To the Editor of the Annals of Electricity, &c.*

*Manchester, September* 17, 1838.

Dear Sir,

I have often during thunder storms noticed that the flashes of lightning do not always appear momentary, but frequently as if there were two distinct flashes, one succeeding the other at an interval of about $\frac{1}{4}''$ of time.

And indeed, this fact cannot have escaped the observation of any attentive person, who has seen the lightning of a distant storm at night; then, the flame of light is in general seen to shine brightly at first, then partly to die away, and at last to end with about the same lustre with which it began; the whole scene taking up in some cases a length of time so palpable as one second. In this case, the lightning has lost so much of its dazzling brilliancy, that the evidence of our senses may safely be trusted.

For my own part I feel quite unable to furnish any hypothesis at all calculated to account for the above phenomenon; I present it therefore to you and your many scientific readers for explanation.

I am, dear sir,
Your obedient servant,
P.

THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND CHEMISTRY;
AND
Guardian of Experimental Science.

APRIL, 1839.

LXXII. *Upon Telegraphic communication, especially by means of Galvanism.* *By* Dr. Steinheil, *Professor of Mathematics and Natural Philosophy, at the University of Munich, &c., &c., &c.*

*(Continued from page 452.)*

## APPENDIX.

### *Description and Figures of the magneto-galvanic telegraph erected between Munich and Bogenhausen, in 1837.*

The telegraph is composed of three principal parts. 1.—A metallic connexion between the stations. 2.—The apparatus for exciting the galvanic current. And 3.—The indicator.

### I. *Connecting wire.*

This so-called connecting wire may be looked on as the wire completing the circuit of a voltaic battery extended to a very great length. What applies to the one holds good of the others. With equal thicknesses of the same metal, the resistance offered to the passage of the galvanic current is proportional to the length of the wire. With equal lengths of the same metal, however, the resistance diminishes inversely with the section; but the conducting power of metals is very different. According to Fechner, copper conducts six times better than iron, and four times better than brass. The conducting power of lead is even lower, so that the only metals which can well vie with each other in their technical use are copper and iron. But now though iron is about six times as cheap as copper it will be requisite to give the iron wire six times the weight of a copper one to gain the same conducting power with equal lengths. We thus see that as far as the

expense is concerned it comes to the same thing whichever of these metals is chosen. The preference will, however, be given to copper, as this metal is less liable to oxidization from exposure to the atmosphere. This latter difficulty may nevertheless be surmounted by simple means, namely, by galvanising it. It would even appear that the simple transmission of the galvanic current when the telegraph is in use is sufficient to preserve the iron from rust; such at least is observed to be the case with the iron portion of the wire used for the telegraph here, and which has already been exposed in all weathers for nearly a twelvemonth.

If the galvanic current is to traverse the entire metallic circuit without any diminution of intensity, the wire during its whole course must not be allowed to come into contact with itself; neither should it be in frequent contact with semi-conductors, inasmuch as a portion of the power called into action takes its course by the shortest way in consequence thereof, whereby the remotest parts are deprived of a portion of the power.

Numerous trials to insulate wires, and to conduct them below the surface of the ground have led me to the conviction that such attempts can never answer at great distances, inasmuch as our most perfect insulators are at best but very bad conductors. And since in a wire of very great length, the surface in contact with the so-called insulator is uncommonly large, when compared with the section of the metallic conductor, there necessarily arises a gradual diminution of the force, inasmuch as the out and the home wire, although but slightly, yet do communicate in intermediate points. It would be wrong to think that this difficulty would be got over by placing the out and the home wire very far apart. The distance between them is, as we shall see in the sequel, almost a matter of indifference. And as we shall never succeed in laying down conductors that are sufficiently insulated beneath the surface of the ground which is always damp, there is but one other course open to us, namely, leading the wire through the air. Upon this plan, it is true, the conductor must be supported from time to time; it is liable to be injured by the evil disposed, and is apt to suffer from violent storms or from ice which forms upon it. As we however have no other method that we can avail ourselves of we must endeavour by suitable arrangements to get the better of these, not immaterial, faults in the best way we can.

The conducting chain of the telegraph erected here, consists of three parts: one leads from the Royal Academy to the Royal Observatory, at Bogenhausen, and back, and the total

length of its wire is 32506 feet. The weight of the copper wire employed amounts to 260 pounds. Both wires (there and back) are stretched across the steeples of the town at a distance of four feet one inch. The greatest distance from support to support is 1279 feet; this is undoubtedly far too great for a single wire, inasmuch as the ice that forms upon it materially increases the weight of the wire itself, and considerably augments its diameter, so that it thus becomes liable to be torn asunder by high winds. Over those places where there are no high buildings, the connecting wire is supported upon tall poles forty or fifty feet long, which are let five feet into the ground, and to the top of which the wire is fastened to a cross bar. At the point where the metal rests there is simply a piece of felt laid, and the wire is made fast by twisting it round the wooden bar. The distance from pole to pole ranges between 640 and 850 feet, but this is far too great, for experience has shown that the wires become considerably stretched by high winds and other causes, and have, in consequence, had to be taken up more than once.*

The conducting wire thus mounted is by no means completely insulated. When, for example, the circuit is broken at Bogenhausen, an induction-shock given in Munich ought to produce no galvanic excitation whatever in the parts of the chain then disconnected. Gauss's galvanometer, however, even then gives indication of a weak current; measurements indeed go to show that this current goes on increasing as the point at which the interruption of the stream is made recedes from the inductor. The absolute amount of this current is not constant. Generally it is strongest when the weather is damp. When there are heavy showers of rain, it may be fairly said to be five times as strong as when the weather is settled dry. At moderate distances of a few miles this small loss of power is of almost no importance, and that the more as the construction of the inductor places currents of almost any strength we choose at our command. When the distance however amounted to upwards of 200 miles, the greatest part of the effect would be dissipated. In such cases much greater precaution must be taken with regard to the points of support of the metallic circuit.

* All these evils would be got over by making the connexion by at least a triple strand of metal, and not by a single wire, supporting it at intervals of 300 feet, and giving it a tension not exceeding one-third of what it will bear without giving way. This, however, in the experimental telegraph erected here, was not practicable, for reasons into which we cannot enter.

When thunder storms occur, atmospheric electricity collects on this semi-insulated chain as upon a conductor, but the passage of the galvanic current is not at all affected thereby.* Quite recently I made the discovery that the ground may be employed as one half of the connecting chain. As in the case of frictional electricity, water or the ground may with the galvanic current form a portion of the connecting wire. Owing to the low conducting power of these bodies compared with metals it is necessary that at the two places where the metal conductor is in connexion with the semi-conductor, the former should present very large surfaces of contact. Taking water, for instance, to conduct two million times worse than copper, a surface of water proportional to this must be brought in contact with the copper to enable the galvanic current to meet with equal resistance, in equal distances of water and of metal. For instance, if the section of a copper wire is 0.5 of a square line it will require a copper plate of sixty-one square feet surface in order to conduct the galvanic current through the ground, as the wire in question would conduct it. But as the thickness of the metal is quite immaterial in this case, it will be always within our reach to get the requisite surfaces of contact at no great expense. Not only do we by this means save half the conducting wire, but we can even reduce the resistance of the ground below what that of the wire would be, as have been fully established by experiments made here with the experimental telegraph.

A second portion of the conducting chain leads from the Royal Academy to my house and observatory in the Lerchenstrasse. This conductor is of iron wire, its length, there

* An occurrence may be mentioned here as a warning for the future. During a severe thunder storm on the 7th July, 1838, a very strong electric spark darted at the same instant through the entire conducting chain, and there was simultaneously produced at the indicator, that is fitted up in my room, a sound like the cracking of a whip. At the same time the lower toned bell of the indicator emitted a sound owing to the deflection of the needle, and the blow was so hard that the points on which the magnetic bar plays were injured. The same phenomenon was observed also at one of the other stations. As the deflecting power of frictional electricity is very inconsiderable with respect to magnets, the above occurrence indicates the presence of a vast quantity of electricity. It can only have arisen from the electricity of the earth having at that moment made its way to that collected in the wire. Whether this was brought about through the lightning conductors in the neighbourhood, or the imperfect insulation of the points of support, cannot be well made out.

and back, is 5745 feet, and it is stretched over steeples and other high buildings, as has already been described. Lastly, a third portion of the chain running through the interior of the buildings connected with the Royal Academy, leads to the mechanical workshop attached to the cabinet of Natural Philosophy. It is composed of a fine copper wire 958 feet long, let into the joinings of the floor, and in part imbedded in the walls. These three portions together compose a line, returning into itself and into which the apparatus for generating the galvanic current, and also the indicator, are inserted.

2. *Apparatus for generating the galvanic current.*

Hydro-galvanism or the galvanic current generated by the action of the voltaic pile is by no means fitted for traversing *very long* connecting wires, because the resistance in the pile, even when many hundred pairs of plates are employed, would be always inconsiderable compared with the resistance offered by the wire itself.

The principal disadvantage however, attendant on the use of the pile or trough apparatus, is the fluctuations of their current joined to the circumstance of their becoming very soon quite powerless, and requiring to be taken to pieces and put together again. The extremely ingenious arrangement of Morse is likewise subject to this inconvenience. All this however is got over when one, to generate the current, has recourse to Faraday's important discovery of induction, that is to say, by moving magnets placed in the neighbourhood of conducting wires. The better way however is, not to move the magnets as Pixii does in his electro-magnetic apparatus, but rather to give motion to the multipliers placed close to a fixed magnet. The arrangement that Clarke has given to the multiplier is the one which, with some modifications, has been adopted. Assuming on the part of our readers a general knowledge of the principles of the apparatus, we here confine ourselves to explaining how it has been adapted to purposes of telegraphic intercourse.

The magnet is composed of 17 horse-shoe bars of hardened steel. With its iron armature its weight is about 74 lbs., and it is capable of supporting about 370 lbs. Between the arms of the magnet there is fastened a piece of metal, supporting in its centre a cup provided with adjusting screws, and which serves as a support for the axis of the coils of the multiplier. The coils of the multiplier have in all 15000 turns of wire. A metre (3 feet 3.3708 inches English) of this wire weighs 15½ grains, and it is twice bespun with silk. Its two ends, which are insulated, are passed up through the interior of the

vertical axis of the multiplier, and then terminate in two hook-shaped pieces, as may be seen in Plate XV, figs. 8 and 9. In order to secure perfect insulation, the vertical axis fig. 8, was bored out hollow. Into this hole there are let in from above two semicircular rods of copper which are prevented touching by a strip of taffeta fastened between them with glue; and these again are kept from touching the metallic axis by winding taffeta round them. In each of these little strips of metal there is, above and below, a female screw cut. In the lower holes small metal pins are screwed in, to which the ends of the multiplier are soldered securely on. While in the upper holes, as may be seen distinctly in figs. 9 and 10, there are iron hooks screwed in. These hooks therefore form the terminations of the multiplier wires of the coils of the inductor. They here turn down, fig. 15, into two semicircular cups of quicksilver that are separated by a wooden partition. From these cups of quicksilver there proceed connexions, J, J, figs. 8 and 13, towards the wires, and they therefore may be considered as forming a part of the chain. The quicksilver owing to its capillarity, stands at a higher level in these semicircular cups than are the partitions, so that the terminal hooks of the wires of the multiplier pass over these partitions without touching them when the multiplier is made to turn on its axis. One sees that the hooks thus are brought into other cups of quicksilver at every half-turn of the multiplier, in consequence of which the galvanic current preserves its sign as long as the multiplier is turned in one direction, but it changes its sign on the motion being reversed. This commutation, which it may be remarked may be established without the use of mercury by the contact of strips of copper that act like springs, is found to answer completely. There are besides two other arrangements which we must not allow to pass unnoticed.

The galvanic current, as we shall see in the sequel when treating of the indicator, should only be permitted to be in action during as short a period as possible, but during that interval should have the greatest intensity we can command. The terminal hooks of the wires dip into the quicksilver only at the place where it forms pools that advance towards each other at the centre, and where the current is at its greatest intensity, see figs. 13, 14, and 15. Fig. 15, shows the position that the inductor has when the terminal hooks first dip into the cups. In all other positions of the inductor it should however form no part of the chain, otherwise the signals made at the other stations will be repeated by its own multiplying wire; and this becomes of the more moment the greater the

resistance in the inductor is. In order therefore to cut off the inductor when in any other position than shown at fig. 15, there is a wooden ring adapted to the axis of rotation of the inductor, see figs. 11 and 12. This ring is encircled with a copper hoop, and into this latter two iron hooks are screwed. These hooks dip down into the semicircular cups of quicksilver as shown at fig. 14. At the moment however that they are passing across the wooden partition, the hooks of the inductor, which are at right angles to them, dip into the cups. When the hooks of the multiplier are in contact with the quicksilver the connexion with the hooks for diverting the current is broken. In every other position the connexion through the hooks of the multiplier is interrupted, while it is established through the others; whence it naturally follows that the current on being transmitted from any other station passes directly through the latter hooks, or, in other words, crosses directly from one quicksilver cup to the other and is not forced to traverse the wire of the inductor for that purpose. In order to put the inductor in motion without trouble, there is a fly bar terminating in two metal balls fastened horizontally on to its vertical axis, Plate XIV, figs. 1 and 2. the quicksilver being scattered about, owing to the hooks as they dip into it when the multipl rapidly, a glass cylinder is fitted on to this part of fig. 1. At every half turn is seen the passag as the hooks of the multiplier leave their cups o

If we choose to give up the phenomena of t thing noways necessary to the employment of the instrument as a telegraph, the inductor will admit of a far more simple construction. It will then merely be necessary to place the commutator directly above the anker, and to let the axis of rotation pass farther up in the neck, in the direction of the fly bar. It then becomes unnecessary to bore the axis out, but the ends of the multiplier are at once fastened by twisting on to two plates of copper, and these copper plates are let into a wooden ring directly opposite each other. The wooden ring is placed upon the vertical axis and made fast to it by clamps. Externally this ring is, in addition to the above-mentioned plates, provided with an arc of copper let into it which acts as a contact-breaker, and two ends of the chain that the current has to traverse have the form of permanent springs that keep pressing against the wooden rings directly opposite each other. By this means with this arrangement also the ends of the inductor are in metallic communication with the chain only during a small portion of each revolution, while during the rest of the time the connecting arc brings the ends of the

chain into direct contact. This construction, in which quicksilver is entirely dispensed with, is, on account of its greater simplicity and durability, preferable to the arrangement first described. The apparatus of the stations at Bogenhausen and in the Lerchenstrasse are thus constructed.

### 3. The Indicator.

We have shown in the preceding paper that our aim is to employ the current developed by the inductor and led through the conducting chain, that when passed across magnetic bars that are delicately suspended it may cause them to be deflected as was discovered by Oersted. These deflections, if we wish to give the signals in quick succession, must follow each other with the greatest rapidity, and should therefore be powerful. This points out to us the size we should give the magnetic bars we wish to deflect. They must not however be made too small, as in that case the mechanical force arising from their deflection is not strong enough to be directly applied to striking upon bells or any other similar purpose. The deflections are, as is well known, taking the force of the current to be the same, the stronger, the greater the number of turns in the multiplier, or in other words, the oftener the wire is led along the magnetic bar. The size of the diameter of the separate turns, as we know, only exerts an influence inasmuch as it adds to the entire length of the connecting wire. The indicator therefore is a multiplier, whose two ends connect it with the conducting chain, and within which the bar to be deflected is placed. It must be borne in mind that the thinner the wire of the multiplier is, the larger its coils are; and the more turns they make, the greater is the resistance to the current throughout the entire chain.

Figs. 16 and 17, Plate XV, represent the vertical and horizontal sections of an indicator containing two magnets, moveable on their vertical axis, and which from their construction are applicable both to striking bells and also to noting down a type composed of dots. Into the frames of the multiplier, which are made of soldered sheet brass, fig. 16, there are soldered two smaller cases for the reception of the magnets, and which allow of the free motion of their axes. Above and below they have threads cut in them for the reception of four screws in holes, on the ends of which the pivots of the axes turn. By means of these screws the position of the bars may be so regulated that their motion is perfectly easy and free. In the frames of the multiplier there are 600 turns of the same insulated copper wire as was employed for the inductor. The commencement and the end of this wire are

shown at M M, fig. 16. The magnetic bars are, as the figure shows, so situated in the frame of the multiplier that the north pole of the one is presented to the south pole of the other. To the ends which are thus presented to each other, but which owing to the influence they mutually exert cannot well be brought nearer, there are screwed on two slight brass arms supporting little cups, figs. 17 and 18. These little cups, which are meant to be filled with printing ink, are provided with extremely fine perforated beaks that are rounded off in front. When printing ink is put into these cups it insinuates itself into the tube of these beaks owing to capillary attraction; and without running but forming at their apertures a projection of a semi-globular shape. The slightest contact suffices therefore for noting down a black dot. When the galvanic influence is transmitted through the multiplying wire of this indicator both magnetic bars make an effort to turn in a similar direction upon their vertical axes. One of the cups of ink would therefore advance from within the frame of the multiplier while the other would retire within it. To prevent this, two plates are fastened at the opposite ends of the free space that is allowed for the play of the bars, and against which the other ends of these bars press. Only the end of one bar can therefore start out from within the multiplier at a time, the other being retained in its place. In order to bring the magnetic bars back to their original position as soon as the deflection is completed, recourse is had to small moveable magnets, whose distance and position is to be varied till they produce the desired effect. This position must be determined by experiment, inasmuch as it depends upon the intensity of the current called into play.

If this apparatus be employed for producing two sounds easily distinguishable to the ear by striking on bells, it will be right to select clock bells or bells of glass, both of which easily emit a sound, and whose notes differ about a sixth. This interval is by no means a matter of indifference. The sixth is more easily distinguished than any other interval; fifths and octaves would be frequently confounded by those not versed in such matters. The bells are to be supported on little pillars with feet, and their position with respect to the bars and likewise their distance from them is to be determined by experiment. The knobs let into the bar that strike on the bells must give the blow at the place which most easily emits a sound. These hammers however are not to be too close to the bells, as in that case a repetition of the signal can easily ensue. A few trials will soon get over this difficulty. If the indicator is to write down the signals a flat surface of

paper must be kept moving with a uniform velocity in front of the little beaks above mentioned. The best way of doing this is to employ very long strips of the so-called endless paper which is to be wound round a cylinder of wood and then cut upon the lathe into bands of the suitable width. One of these strips of paper must be made to unwind itself from a cylinder, pass close in front of the cups, run along a certain distance in a horizontal position, so that the dots noted down may be read off, and lastly wind itself up again on to a second cylinder. This second cylinder is put in motion by clock work, the regularity of whose action is insured by a centrifugal fly wheel. A longitudinal section of the entire arrangement is shown at fig. 1, Plate XIV. Fig. 2 represents it as seen from above. At the corners of the frame over which the ribbon of paper is led, there are placed two moveable rollers to diminish the friction. This frame moreover admits of being advanced towards the cups or withdrawn from them, so that the most proper position to give it can be ascertained by experiment. It is evident that the same magnetic bars cannot be at once employed for striking bells and for writing, the little power they exert being already exhausted by either of these operations. But to combine them both, all we have to do is to introduce a second indicator into the chain. By thus increasing the number of the indicators the loudness of the sounds of the bells can be augmented at pleasure; this can however only be done at the cost of an increased resistance in the chain. In order that this may be increased by the indicator as little as possible it would in future be better that its coils should be made of very thick copper wire or of strips of copper plate.

The above description will enable those who are familiar with such subjects to construct the apparatus for themselves. We have yet to add a few words upon

*The way of putting the Apparatus together.*

Plate XIV, fig. 1, represents the longitudinal section of a pyramidal table standing on the floor of the room and containing the whole apparatus. Fig. 2 shows the same as seen from above. The wires from Bogenhausen, those from the Lerchenstrasse, the ends of the indicator, and the wires from the quicksilver cups of the inductor, or in other words, the two ends of its multiplier, all meet together at the centre of the table as seen at fig. 2, of Plate XIV. They are here brought into connexion with eight holes filled with quicksilver, made in a disc of wood as shown at fig. 3, Plate XV. The course that the current we call forth will take depends upon the respective connexion of these eight holes with each other.

For instance, supposing them to be connected together by four pieces of bent copper wire, as shown at fig. 3, the current would pass through the whole apparatus, and also the entire chain. Establishing however the connexion as shown at fig. 6, would cut off the Bogenhausen station, and would at once transmit the current direct from the inductor through the multiplier of the indicator and through the Lerchenstrasse station. Supposing this figure turned round 180 degrees we should have the Lerchenstrasse station cut off, and the current would pass through Bogenhausen. A third system of connexions is shown by the copper wires represented in fig. 7. In this position of the sketch the inductor and the multiplier would be in direct communication while the two stations at Bogenhausen and in the Lerchenstrasse would be cut off. But by turning this figure 90 degrees we should connect these two stations while we broke off the station in the Academy. Copper wires serving to establish these three systems of connexion and the combinations are laid down upon the under surface of the wooden cover of the commutator, as seen at fig. 4. There are 24 wires projecting downwards from this lid. Only eight of them, however, ever come into use at once; so that there must be sixteen other holes made in the lower disc of wood for the reception of the wires not in use, and having no quicksilver poured into them. It is thus in our power to direct the course of the current as we choose, and the systems concerned are indicated upon the upper surface of the cover of the commutator by engraved letters, see fig. 2, Plate XV., this cover containing the different modifications of the systems of connexion, as shown at fig. 4. Changing the position of this cover round the central pin springing from the table enables us to vary the direction of the current in any manner we like. The use of quicksilver cups in the commutator may of course be replaced by conically turned copper pins. This has indeed been done at the Lerchenstrasse and the Bogenhausen stations.

We shall conclude by a few remarks upon

*The application of this apparatus to Telegraphic communication.*

We know from what has preceded that at every half turn of the fly-bar from right to left one of the bars is deflected. I have so connected the terminations of the wires that every time this movement is repeated the high toned bell should be struck at all the stations. Standing at the side BB, and turned towards the indicator, one immediately perceives the beak imprint a dot upon the ribbon of paper as it moves along.

The intervals of time between the successive repetitions of this sign are represented by the respective distances between the dots that follow in a line upon the paper. On turning the fly-bar from left to right towards the operator, the deep toned bells ring and the second ink cup marks down a dot upon the paper as before, not, however, upon the same line with the former dots, but on a lower one. *High* tones are therefore represented by the *upper* dots, and *low* tones by the dots of the *lower* line, as in writing music. As long as the intervals between the separate signs remain equal they are to be taken together as a connected group, whether they be pauses between the tones, or intervals between the dots marked down. A longer pause separates these groups distinctly from each other. We are thus enabled, by appropriately selected groups thus combined, to form systems representing the letters of the alphabet or stenographic characters, and thereby to repeat and render permanent at all parts of the chain, where an apparatus like that above described is inserted, any information that we transmit. The alphabet I have chosen represents the letters that occur the oftenest in German by the simplest signs. By the similarity of shape between these signs and that of the Roman letters, they become impressed upon the memory without difficulty. The distribution of the letters and numbers into groups consisting of not more than four dots is shown at Plate XIV, fig. 1.

JULIAN GUGGSWORTH.

*[illegible],*
*Nov. 24, 1838.*

---

## LXXIII. *The variation of the Compass occasioned by a non-coincidence between the pole of the earth and the pole of the ecliptic, communicated to the Editor of the Annals of Electricity. By* Edward Nayler, Esq., *First Lieutenant of the Royal Marines.*

There are two modes by which a question in natural philosophy may be investigated: one derived from the consideration of some subject setting forth similar phenomena to those which we desire to investigate; and another proceeding directly to the analysis of the phenomena, as found to exist in the subject itself: which mode of enquiry, wherever it be practicable, appears the most direct and least liable to error. There are some instances in which this direct method cannot be pursued. Newton has explained the motions of the planets by investigating the laws of gravity, as found to exist in

nature; developing its effects, he imputes the motions of the planets to the agency of its laws; causing that which was within his reach, to explain that which lay beyond. But it does not follow that this analogical course of reasoning must necessarily be pursued, to the exclusion of the more direct, when the subject of investigation may seem to require it. I have, therefore, felt myself at liberty, in an enquiry concerning the variation of the compass, to pursue the direct method; that is to go immediately to the consideration of certain facts about which there can be no dispute; and thence endeavour to deduce, if not the laws which influence the compass, at least some of the circumstances upon which its deflections appear to depend. The facts about which there can be no dispute are as follows:—

First, the needle does not point to the true north of the earth; if it did, there would be no variation in its direction from the true north. Consequently the variation of the needle, or its deflection from the true north, proves that the place to which the needle is attracted, is different in position from the true north of the earth.

Secondly, the place to which the needle is attracted, appears not always to have a fixed and uniform position, in relation to the true north of the earth; if it had, the variation would never change in quantity and character, being sometimes east and sometimes west; but would be fixed and uniform at a given place; but it is known not to be so: consequently, by these changes, the place to which the needle is attracted is proved not always to have a fixed position in relation to the true north of the earth.

Thirdly, we find on certain occasions at particular places, the needle has ceased for a time to have any variation in its direction from the true north of the earth. The place then to which the needle at that time was attracted, in relation to the given place, coincided with the true north of the earth. If the place to which the needle at that time was attracted, and the true north of the earth had not been so situated, in relation to each other, as that the needle when directed to the one pointed to the other, there would have been a variation in the direction of the needle from the true north; but there was no variation in the direction of the needle from the true north of the earth, at the given place; and therefore the place to which the needle at that time was attracted was so situated in relation to the true north, as that the needle pointing to the place of its then attraction, was directed at the same time to the true north of the earth.

Fourthly, the changeable nature of variation, both as to quantity and character, east and west, noticed in the second of the preceding articles, proves that either the true north of the earth, or the place to which the needle is attracted, is in motion. Now there is no foundation for a belief, that the true north or pole of the earth, is in motion to an extent sufficient to account for the changes experienced, either in the quantity or character of the variation of the needle. If the pole of the earth be in motion, to an extent commensurate with the changes experienced in variation, the nutation of the earth's axis must be greater than it is perceived to be, and the change in the obliquity of the ecliptic, must also be greater than perceived. Consequently the nutation of the earth's axis being insufficient to account for the changes experienced in the quantity and character of variation, we are bound to superadd to the known motion of the earth's axis, a quantum of motion, properly belonging to the place to which the needle is attracted; that the two motions together may be sufficient to account for the changes perceived in variation; or, perhaps, for all the purposes of an approximate enquiry, we may be allowed to consider the pole of the earth at rest, and conceive the motion necessary to account for the changes in the quantity and character of variation, as belonging properly to the place to which the needle is attracted.

Finding what has been stated above, namely, that the needle does not point to the true north of the earth, but to some place, distinct and apart; and that the place to which the needle is attracted, has not always the same position in relation to the true north, but is in motion; we may naturally enquire, if there be anything in the phenomena of the earth's motion, calculated to explain these points, when we shall perceive, first, that the pole of the earth, and the pole of the ecliptic, do not coincide. Secondly, that the pole of the ecliptic is in motion.

Considering the two last-mentioned facts, we perceive if a line be projected from the pole of the ecliptic to the earth, it will terminate on some point upon the arctic circle; and if the pole of the ecliptic be in motion, the terminating point of a line projected from it to the earth must be in motion; and therefore according to what is found in the phenomena of the earth's motion, a point may be imagined to exist on the arctic circle which shall be in motion.

Again, if a line be projected from the pole of the ecliptic to the earth, it will not only terminate on the arctic circle, but also on some particular meridian; and as all meridians pass

through the arctic circle, and intersect each other in the pole of the earth, the terminating point of the line projected from the pole of the ecliptic will be on a meridian terminating in the pole of the earth. And being so, in certain situations things directed to it will also be directed to the pole of the earth. Therefore we perceive another point in the phenomena of the earth's motion, analogous to certain facts, which serve to explain some of the circumstances of variation; shewing why in certain cases, the place to which the needle is attracted, may coincide with the true north or pole of the earth, and there be no variation.

It has been objected to such a course of enquiry respecting the variation of the compass, that modern modes of investigation into the phenomena of nature reject hypothetical reasoning; and because no reason can be exhibited as to a cause why the needle should be attracted to a point in the arctic circle, such an assumption, however it may respond to the phenomena of variation as found to exist in nature, is yet undeserving of consideration; being unworthy of the closer reasoning of the moderns. But if it be an hypothesis, it is grounded, as we have seen, on certain indisputable facts, observable in the variation of the compass; and also connected with the known and admitted phenomena of the earth's motion; namely, that the pole of the earth and the pole of the ecliptic do not coincide; from which we have seen a variation would result. Secondly, that the pole of the ecliptic is in motion; and consequently variation would change its nature and quality, regulated by the motion of the pole of the ecliptic; therefore it cannot be an hypothesis deserving of no consideration, for if the place to which the needle be attracted, be on the arctic circle, and be in motion, regulated by the known motion belonging to the pole of the ecliptic, it may be proved by the quantities of variation exhibited in nature, and the changes they undergo at different places. We are therefore, it is hoped, warranted in prosecuting such an enquiry, for the purpose of seeing what peculiarities variation would exhibit in different parts of the world, if the needle was attracted to a point on the arctic circle, derived from the idea of a line projected from the pole of the ecliptic to the earth, and thence further continuing to enquire if those peculiarities agree with what is discovered in nature at different periods and different places.

For the purpose of prosecuting these enquiries let *a b g C*, fig. 1, Plate XVI., be the earth; *b c*, the equator; A D, the ecliptic; *s*, the point on the arctic circle to which the needle may be supposed to be attracted; and *a*, the north of the

earth or the extremity of the earth's axis; *a g*, the axis of the earth; and *s o*, the magnetic axis, or the projected line from the pole of the ecliptic continued through the earth's centre. Then *a x d g*, is a meridian, and *s x o*, is a magnetic meridian. Now anything situated on the meridian, *a b g* directed to *s* will point to *a*; consequently on that meridian there will be no variation. Again, anything situated on the meridian *a* A C, being directed to *s*, will also point to *a*; consequently there will be no variation on the meridian *a* A C. Again, the meridians *a s b*, and *a* A C are 180° distant from each other; therefore if the needle be attracted to *s*, a point in the arctic circle, there will be two meridians 180° distant from each other, on which there will be no variation. One of these meridians *a s b*, will be that on which the magnetic pole is seated, and the other that where variation changes its character.

Variation changes its character after passing the meridian *a* A C. In passing from the meridian *a s b* to *a* A C, the variation will be westerly; because the magnetic pole *s*, estimated in direction at any part of *k x l* is to the left of the true pole *a*. But in passing round the earth on the other side of the meridian *a* A C, the magnetic pole *s*, will be to the right of the true pole *a*; consequently the variation will be easterly. Again, in passing along from *k* to *x*, and *l* departing from *k*, variation will be in small quantities westerly, gradually increase, attain its highest quantity or maximum, then diminish and vanish at *l*. Having passed *l* on the meridian *a* A C on the other side, variation becomes easterly, continues so increasing till it attains its maximum, then diminishes, and vanishes at *k*.

Moreover if *s* the magnetic pole be conceived to move to *r*, influenced as before stated by the movement of the equinoctial pole, any place *x* will experience all those changes before mentioned. Whilst *s* remains on the meridian *a k b*, there will be no variation on that meridian, or on *a* A C. As *s* approaches the meridian *a x*, westerly variation will lessen on *a x*; when *s* shall be on the meridian *a x*, there will be no variation on that meridian; *s* having left *a x*, proceeding towards *l*, variation will become easterly on *a x*, continue to increase as *s* advances towards *r*, and still proceeding beyond *r*, will attain its maximum, diminish and vanish on reaching a meridian 180° distant from *a x*.

In the southern hemisphere, the same reasoning obtains with this difference, namely, in all places between the meridians *b* D *g*, and C *o g*, there will be easterly variation; because the magnetic pole *o*, is to the right of the true pole *g*,

But all the other peculiarities would be alike in either hemisphere. That is to say C o g, would be the meridian on which the southern magnetic pole would be situated, and there would be no variation there; for anything on that meridian directed to o, the south magnetic pole, would point to g, the south pole of the earth. Again b D g, would be the meridian in the southern hemisphere where variation would change its name, and there would be no variation on that meridian; for anything directed to o would point to g also. In passing from one of these meridians b D g to C b g, variation would increase, and decrease in the manner before stated, in relation to the northern hemisphere; but with an opposite character from that in the northern hemisphere. If in the northern hemisphere between a s b, and a A C, it had been westerly variation, in the southern hemisphere between b D g, and C o g, it would be easterly variation. There are other considerations, belonging to the subject of variation in the southern hemisphere, and on the equator, which we shall not at present consider, but reserve them until a future occasion.

These, then, are the peculiarities variation would assume if the needle was attracted to a point in the arctic circle. We have now to enquire, whether these peculiarities agree with what has been discovered in nature, at different times and different places. And if it should appear that there is a remarkable agreement between the peculiarities above noticed, and the quantities of variation found in nature; and also if it should be found that the movement of the magnetic pole may be estimated in a certain relation by the known motion of the equinoctial pole, it is hoped the enquiry may not be considered unworthy of investigation; though no reason at present can be assigned, why the needle should be attracted to a point on the arctic circle, correspondent to the pole of the ecliptic.

In order to institute a comparison between the quantities of variation found in nature, and those that would be generated by the reasoning adduced, let the triangle *s x a* be considered as a plain triangle *s a* = to the obliquity of the ecliptic, *x a* = to the co-latitude of the place of observation (*x*); *s x a* = to the angle of variation at the plane of observation (*x*); and *s a x* = to the distance of the meridian on which *x* is situated, from the meridian *a b g*, on which the magnetic pole *s* is seated. I would consider the triangle *s x a* as a plain triangle; from a belief that the needle takes the shortest course to its place of attraction, and therefore the side *x s* might be indicated by the chord of the arc *x s*, and so with the other sides respectively.

But as there are two meridians, on which according to former reasoning, there would be no variation, it may be well for the purpose of perspicuity, to distinguish these meridians by some peculiar appellation. The meridian $a\,b\,g$ is that on which the magnetic pole $s$ is situated, consequently the angle $s\,a\,x$ expresses the distance of the meridian $a\,b\,g$, from the meridian $a\,d\,g$, therefore we shall call the meridian $a\,b\,g$ the north magnetic meridian, and the angle $s\,a\,x$ the north magnetic distance of $x$ the place of observation. Again, $a$ C $g$ is the meridian, on which there is also no variation, according to our former reasoning; being that meridian on which variation ceases, and beyond which, it begins to change its name. The meridian $a$ C $g$, may therefore be called the ante-magnetic meridian, and the angle $x\,a\,r$ will be the supplemental angle of the magnetic distance ($\angle\, s\,a\,x$) for the $\angle\, s\,a\,x + \angle\, x\,a\,r = 180^{\circ}$. We may, therefore, call the $\angle$ $x\,a\,r$ the ante-magnetic distance, or the supplemental magnetic distance.

Now the quantity of variation at the place $x$, might be estimated if the $\angle\, s\,a\,x$, or the magnetic distance of the place of observation ($x$) was known; for there would then be given, the angle $s\,a\,x$ equal to the magnetic distance of the place of observation ($x$); the side $s\,a$ equal to the obliquity of the ecliptic; and the side $a\,x$ equal to the co-latitude of the place of observation ($x$); that is two sides $s\,a$, $x\,a$, and an included angle $s\,a\,x$, to find the angle $s\,x\,a$. When, for reasons before stated, treating the question as a case in plane trigonometry, the following formula would give the angle $s\,x\,a$, in places either within or without the arctic circle:

Twice the nat. sin. of half the co-lat. + twice the nat. sin. of half the obliquity of the ecliptic,

Is to twice the nat. sin. of half the co-lat. — twice the nat. sin. of half the obliquity of the ecliptic.

As the tan. of half the supplemental magnetic distance

Is to the tan. of half the difference of the supplemental magnetic distance (or A).

Then half the supplemental magnetic distance minus A equals the angle of variation, in places situated without the arctic or antarctic circle: and half the supplemental magnetic

distance + A equals the angle of variation, at places situated within the arctic or antarctic circles.

It has been said, the variation might be estimated by the above formula, if the angle $s\,a\,x$, or the manetic distance of the place $x$ (any place of observation), was known. But it is evident the quantities of variation, generated under the reasoning before detailed, might be obtained for any given parallel of latitude, by assuming the $\angle\, s\,a\,x$ as successively equal to 1°, 2°, 3°, 4°, &c. to 180° when variation would cease, it being the meridian $a$ A $g$. Again continuing the angle $s\,a\,x$, beyond that meridian in a descending series as equal successively to 179°, 178°, 177°, &c. to 0°, when again on the meridian $a\,b\,g$ there would be no variation. Now the quantities thus obtained for every degree of latitude would embrace all the features of variation throughout 180 degrees of longitude, according to the reasoning before detailed. Again if variation could be thus obtained for one parallel of latitude it might be obtained for every one. Then the quantities thus procured, being arranged in tables, according to the angle of their distance ($s\,a\,x$) from the magnetic meridian, in the several parallels of latitude, for either hemisphere, would indicate all the peculiarities of variation for every part of the world; and thus the views of variation suggested by our former reasoning be brought to the test of observation. In order to effect such an object during Capt. Ross's first voyage, tables of the above description were constructed; and on his return the observations made during his voyage were compared, and results obtained, such as shown in the following table. The fourth column shows the distance of the place of observation, from the meridian $a$ A $g$, when variation changes its name. The fifth column shows the dist. of the meridian of London, from that of $a$ A $g$. The sixth column is the distance of London from the meridian $a$ A $g$, derived from a mean taken of all the observations.

| Latitude observed North. | | | Longitude observed West. | | | Variation observed West. | | | Distance of place of observation from meridian *a* A *g* Fig. 1st. | | | Distance of London from the meridian *a* A *g* Fig. 1st. | | | Distance of London from meridian *a* A *g* by mean of all the Observations. | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| ° | ′ | ″ | ° | ′ | ″ | ° | ′ | ″ | ° | ′ | ″ | ° | ′ | ″ | ° | ′ | ″ |
| 70 | 55 | " | 53 | 53 | " | 79 | " | " | 131 | 59 | " | 78 | 6 | " | 67 | 57 | 53 |
| 74 | 2 | " | 58 | 45 | " | 80 | 1 | " | 122 | 5 | 39 | 63 | 20 | 39 | 67 | 57 | 53 |
| 75 | 28 | " | 60 | 36 | " | 88 | 25 | " | 126 | 39 | 49 | 66 | 3 | 49 | 67 | 57 | 53 |
| 75 | 28 | " | 60 | 36 | " | 88 | 53 | " | 127 | 8 | 23 | 66 | 22 | 23 | 67 | 57 | 53 |
| 75 | 58 | 56 | 64 | 37 | 21 | 91 | 18 | " | 127 | 58 | 11 | 63 | 20 | 50 | 67 | 57 | 53 |
| 75 | 50 | " | 64 | 41 | " | 90 | 18 | " | 127 | 25 | 54 | 62 | 44 | 54 | 67 | 57 | 53 |
| 75 | 50 | 30 | 64 | 47 | " | 91 | 32 | " | 128 | 38 | 21 | 63 | 51 | 21 | 67 | 57 | 53 |
| 75 | 54 | " | 65 | 32 | " | 92 | 44 | " | 129 | 38 | 9 | 64 | 6 | 9 | 67 | 57 | 53 |
| 76 | 25 | " | 71 | " | " | 103 | 10 | " | 137 | 28 | 53 | 66 | 28 | 53 | 67 | 57 | 53 |
| 75 | 25 | " | 71 | " | " | 103 | 10 | " | 142 | 24 | 42 | 69 | 24 | 42 | 67 | 57 | 53 |
| 76 | 28 | 30 | 73 | 19 | 45 | 102 | " | " | 136 | 19 | 24 | 62 | 59 | 39 | 67 | 57 | 53 |
| 76 | 54 | " | 74 | 20 | " | 103 | " | " | 135 | 57 | 19 | 61 | 37 | 19 | 67 | 57 | 53 |
| 76 | 46 | 15 | 75 | 21 | 45 | 102 | " | " | 135 | 19 | 35 | 59 | 57 | 50 | 67 | 57 | 53 |
| 76 | 32 | 45 | 76 | 32 | 45 | 105 | " | " | 138 | 38 | 14 | 62 | 5 | 29 | 67 | 57 | 53 |
| 76 | 32 | 15 | 76 | 54 | 45 | 108 | " | " | 141 | 4 | 40 | 64 | 9 | 55 | 67 | 57 | 53 |
| 76 | 10 | " | 78 | 30 | " | 109 | 58 | 30 | 143 | 34 | 28 | 65 | 4 | 28 | 67 | 57 | 53 |
| 76 | 44 | " | 78 | 28 | " | 110 | " | " | 142 | 5 | 56 | 63 | 37 | 56 | 67 | 57 | 53 |
| 76 | 4 | " | 72 | 28 | " | 110 | " | " | 143 | 1 | 53 | 64 | 33 | 53 | 67 | 57 | 53 |
| 74 | 59 | " | 78 | 1 | " | 113 | " | " | 149 | 4 | 28 | 71 | 2 | 58 | 67 | 57 | 53 |
| 74 | 19 | 30 | 78 | 33 | " | 110 | " | " | 148 | 53 | 43 | 70 | 20 | 43 | 67 | 57 | 53 |
| 74 | 3 | " | 80 | 37 | " | 114 | " | " | 152 | 22 | 14 | 71 | 45 | 14 | 67 | 57 | 53 |
| 74 | 3 | " | 81 | 28 | " | 114 | " | " | 152 | 22 | 14 | 70 | 54 | 14 | 67 | 57 | 53 |
| 73 | 37 | " | 77 | 25 | " | 110 | " | " | 151 | 1 | 30 | 73 | 36 | 30 | 67 | 57 | 53 |
| 73 | 45 | " | 74 | 10 | " | 105 | 51 | " | 147 | 37 | 44 | 73 | 27 | 44 | 67 | 57 | 53 |
| 73 | 45 | " | 74 | 10 | " | 106 | 51 | " | 148 | 22 | 10 | 74 | 12 | 10 | 67 | 57 | 53 |
| 70 | 34 | 30 | 67 | 46 | 45 | 75 | " | " | 128 | 26 | 22 | 60 | 39 | 37 | 67 | 57 | 53 |
| 70 | 34 | 30 | 67 | 40 | 15 | 75 | " | " | 128 | 34 | 11 | 60 | 53 | 56 | 67 | 57 | 53 |
| 67 | 27 | " | 64 | 17 | " | 69 | " | " | 132 | 45 | 30 | 68 | 28 | 30 | 67 | 57 | 53 |
| 66 | 56 | " | 56 | 28 | " | 66 | " | " | 129 | 53 | 14 | 73 | 25 | 14 | 67 | 57 | 53 |
| 67 | 5 | " | 67 | " | " | 67 | " | " | 131 | 59 | 18 | 64 | 59 | 18 | 67 | 57 | 53 |
| 66 | 18 | 30 | 58 | 30 | " | 67 | " | " | 135 | 18 | 9 | 76 | 48 | 9 | 67 | 57 | 53 |
| 65 | 54 | " | 59 | 25 | " | 70 | " | " | 144 | 29 | 42 | 84 | 54 | 42 | 67 | 57 | 53 |
| 62 | " | 15 | 62 | 25 | " | 56 | " | " | 137 | 32 | 37 | 75 | 7 | 37 | 67 | 57 | 53 |
| 59 | 45 | " | 51 | 23 | 30 | 46 | " | " | 113 | 39 | 58 | 72 | 16 | 28 | 67 | 57 | 53 |

In the formation of the tables of variation from which the above distances from the meridian *a* A *g* were obtained, the sides of the angle *s a x*, that is *s a* the obliquity of the ecliptic,

and $xa$ the co-latitude of the place of observation $x$, were not taken as the chords of their respective arcs, but were taken in extent according to the geographical miles contained; this was thought sufficiently accurate for an approximate consideration. Therefore quantities somewhat different would have been obtained if these distances had been worked for, according to the formula before exhibited.

---

## LXXIV. *Facts and observations for the purpose of illustrating a Theory intended to connect the Operations of Nature upon general principles. By* PAUL COOPER, ESQ.

*(Continued from page 502.)*

158. In these experiments, a part if not the whole of the opposition of the platina, proceeding from its negative state compared with hydrogen, is disposed of by bringing in contact with it a body more negative than hydrogen, and, consequently, more nearly approaching its own electrical state. I have already had occasion to observe, that the superior electromotive force produced by the addition of nitric acid, arises from the negative character of the body, whether it be nitrous acid or any other oxide of azote, which upon its decomposition it leaves in contact with the electrode at the cathode (90). If this body and platina be nearly equal in their electrical forces (98), the experiments in which nitric acid was introduced with interposed platina plates will be upon a level with those in which copper was employed with dilute sulphuric acid: in the former, the oxide of azote and platina, and in the latter the hydrogen and copper, being nearly equal in their electrical forces, the metals act merely as conductors and occasion little or no obstruction to the current, except that which after some time arises from a peculiar state of their surfaces. (Researches 1030.) It appears then, that the resistance to the transmission of an electric current by platina plates, is chiefly attributable to their negative state compared with hydrogen (87); that when zinc, which is positive to hydrogen (86), and copper which is nearly equal with it (83), are substituted for platina, the resistance is removed; and that this is also the case when a body nearly approaching platina in its electrical character is substituted for hydrogen at the cathode (90. 98).

159. I will now endeavour to trace to its cause the peculiar state of the surfaces of the interposed plates, to which I have just alluded, and which Dr. Faraday has more fully

described in the following extracts from his Experimental Researches.

"In an experiment in which one voltaic pair and one interposed platina plate were used with dilute sulphuric acid in the cells, fig. 82, the wires of communication were so arranged, that the end of that marked 3 could be placed at pleasure upon paper moistened in the solution of iodide of potassium at $x$, or directly upon the platina plate there. If after an interval during which the circuit had not been complete, the wire 3 were placed upon the paper, there was evidence of a current, decomposition ensued, and the galvanometer was affected. If the wire 3 were made to touch the metal of $p$, a comparatively strong sudden current was produced, affecting the galvanometer, but lasting only for a moment; the effect at the galvanometer ceased, and if the wire 3 were placed upon the paper at $a$, no signs of decomposition occurred. On raising the wire 3, and breaking the circuit altogether for awhile, the apparatus resumed its first power, requiring however, from five to ten minutes for this purpose; and then, as before, on making contact between 3 and $p$, there was again a momentary current, and immediately all the effects apparently ceased." (Researches 1036.)

160. If the apparatus, fig. 82, be left in action for an hour or two, with wire 3 in contact with the plate $p$, so as to allow a free passage to the current, then, though the contact be broken for ten or twelve minutes, still, upon its renewal, only a feeble current will pass, not at all equal in force to what might be expected. Further, if $P^1$ and $P^2$ be connected by a metal wire, a powerful momentary current will pass from $P^2$ to $P^1$ through the acid, and, therefore, in the reverse direction to that produced by the action of the zinc in the arrangement; and after this has happened, the general current can pass through the whole of the system as at first, but by its passage again restores the plates $P^2$ and $P^1$ into the former opposing condition." (Researches 1040.)

161. In the first of these experiments, in which the circuit was completed by placing the wire 3 upon the paper, it is probable that the electrolytic arrangement described in fig. 82, did not take place, at any rate beyond the first cell, and that the current was conducted through cell I I as it would be under ordinary circumstances (110). But when the wire 3 was made to touch the metal of $p$, the circuit was completed by metallic contact, and the current from the platina to the zinc took place upon galvanic principles independent of chemical action (59). The force of this temporary current would produce the arrangement described in

fig. 82, and a corresponding current through the fluid; but the current arising from galvanic action must cease upon the metals being brought to a state of equilibrium with the forces in action, until the circuit be again broken (69. 118). Now, upon the passing of the current, the electrolyte will be brought to the state represented in fig. 83; in which the hydrogen, either wholly or partially separated from its oxygen, will present a negative surface to the induced positive surface of the platina in both cells; and the oxygen a positive surface to the induced surface of the zinc in cell I, and to the similar surface of the platina in cell I I. If, under these circumstances, the separation of the oxygen from the hydrogen be completed, it will unite with the zinc in cell I, and the acid present will dissolve the oxide; but this will not be the case in cell I I, and the arrangement there represented will continue. When, therefore, upon the breaking of the circuit, by the separation of the oxidized zinc in the first cell, it is again completed by a renewal of the arrangement, represented in cell I, fig. 82, the electrolyte in the second cell will remain as represented in cell I I, fig. 83; in which the oxygen presents to the current a surface already positive, and, consequently, incapable of receiving any further supply: and, as its polar state prevents its conducting in the ordinary manner, the current must be altogether suspended. No signs of decomposition, therefore, occurred if the wire, under these circumstances, were placed on the paper at $x$.

162. On breaking the circuit altogether for awhile, the apparatus resumed its first power, requiring, however, from five to ten minutes for this purpose. Upon removing the wire 3, the circuit having been at least partially broken in cell I, the plates would resume their usual state, as upon the breaking of the circuit by the removal of the wire A B in fig. 78. The return to this state, however, would receive some opposition from the arrangement represented in cell I I, fig. 83, and, perhaps, by a partial similar arrangement in cell I; and, therefore, time would be required to produce it. When the wire 3 was left in contact with the plate $p$, for an hour or two, the opposition produced by these causes became more confirmed; for though the contact was broken for ten or twelve minutes, still, upon its renewal, only a feeble current was produced. The arrangement represented in fig. 83, now became more general, and, consequently, its opposition more permanent.

163. This explanation is further confirmed by the experiment in which a reverse current was produced, passing through the acid from $P^3$ to $P^1$, when these plates were connected

by a metal wire. This mode of connecting the plates $P^1$ to $P^2$, would render them with the contents of cell I I, an independent circuit, unconnected with the zinc plate or any other part of the system. Under these circumstances, the hydrogen would induce in the platina with which it is in a negative surface, (platina being negative to hydrogen) and the state thus induced, conducted through the wire from $P^2$ to $P^1$, would induce the opposite state in the latter, on the surface in contact with the oxygen. The state of the plates, therefore, in cell I I, will be brought by this operation to a polar state the reverse of that indicated by the signs prefixed to their respective surfaces in fig. 83; and the current will flow from the negative surface of $P^2$ through the dilute acid to $P^1$: in doing this it will reverse the poles of the electrolyte, as it is represented in fig. 83, and, thus, bring it to the state represented in fig. 82. The separated atoms of oxygen and hydrogen in contact with $P^1$ and $P^2$ will again form particles of water, and the fluid in cell I I will be restored to the electrolytic order represented in fig. 82.

164. This experiment corresponds in principle with that (represented in fig. 85, Plate XI., Vol. 2) in which atoms of oxygen and hydrogen were converted to water by the influence of platina (88). When the plates $P^1$ and $P^2$ are connected by a platina wire, the plates and the wire form, in fact, as it relates to their galvanic action, a single mass of platina; and the circuit formed by the electrolyte is to all intents and purposes a local circuit. The hydrogen induces a negative surface in the part of this metallic mass with which it is in contact, and this, propagated through the metal as in other local circuits, produces a positive surface where it is in contact with the oxygen; the current, then, in accordance with the general law (54), being from the negative to the positive surface, would only tend to confirm the existing polarity of the electrolyte as it is represented in fig. 82; but it would reverse it as it is represented in fig. 83, and increase its intensity as it is represented in fig. 85, Plate XI., Vol. 2; and thus restore the cohesive union of the atoms of oxygen and hydrogen in one case, and produce it in the other. This remarkable and unexpected coincidence, developed by experiments so totally different, is strongly confirmatory of the correctness of the principles upon which the explanations of both are founded.

165. Having applied my theory to a few of the experiments of Dr. Faraday, which appear to me to present the greatest difficulties, I shall next proceed to some experiments of Dr. W. C. Henry, which will illustrate the same principles. I have been led to these experiments by their almost immediately

following the part of Dr. Faraday's that have just engaged my attention, in the work from which they were taken.*

155. The discovery, first made by Dobereiner, that the union of oxygen and hydrogen is induced by the presence of spongy platina at the ordinary temperature of the atmosphere, and the more recent discoveries of Dr. Faraday that plates of that metal, as well as plates of paladium and gold, produced the same effect, led Dr. W. C. Henry to the investigation of the cause of the inferiority of other metals in detaining the union of these gases. In the course of this enquiry he found that the oxidizable metals required to be heated to nearly the boiling point of mercury before they induced the silent union of the gases; and that then it was accomplished by the intermediate oxidation of the metal, which alternately absorbed the oxygen of the air and yielded it to the hydrogen. He, therefore, concluded, that these metals do not determine the direct union of hydrogen with free oxygen at any temperature; but that the conversion, and the incandescence which accompanies it, is due to a succession of alternate reductions and re-oxidations.

167. The temperature at which the union of the gases is determined by these metals, is, however, still lower than that which would enable them to combine spontaneously, although the oxygen, being in the first instance cohesively united to the metal, must be presented to the hydrogen under circumstances which will require it to be disconnected from one body before, or at the same instant, that it is united to the other. I am acquainted with no other means of accomplishing these objects, than by forming circuits in the manner that has been so often described.

Let the metal be copper, and let it be surrounded with oxygen and hydrogen, the metal being at the required temperature. This temperature is higher than what is required to produce the spontaneous oxidation of the metal, though not sufficiently high to produce the spontaneous combustion of the gases (166). Some of the atoms of the copper will under these circumstances be cohesively united to the oxygen; and the particles of oxide thus formed, the oxygen being the positive element, will be presented to the gaseous mixture in the polar state represented in fig. 84.

168. The oxide of the copper will be negative to the atoms of the same metal which are not oxidized (169. 174); and circuits will be formed from one to the other by the surrounding gases, in which the atoms of oxygen and hydrogen, under

* Philosophical Magazine, Vol. VI. p. 354.

the influence of electric affinity (88), will follow each other alternately (See fig. 85 and 86). The elective affinity which is exercised in this case, is regulated by a force precisely similar to that which directs the negative body to the positive surface, and the positive body to the negative surface, in forming electrolytic arrangements.

169. The atoms of copper in figures 85 and 86, are supposed to be parts of the same molecule of the metal; and the arrows are intended to show the direction of the currents. The particles of oxide being negative to the pure metal* (174) induce upon the atoms of the latter positive surfaces; and these surfaces, propagated in the usual manner, terminate in negative surfaces, the development of which must be greatly assisted by the molecular state of the metal. The oxygen, the positive element, is directed towards the negative surface; and it is followed by hydrogen and oxygen, alternately, until the circuit is completed, by an atom of hydrogen, with the particle of oxide. Under these circumstances, the current in accordance with the general law (54), being from the negative surface of the copper, the poles of the gaseous atoms in the circuit in fig. 85, are reversed, and brought into the state represented in fig. 86. In this figure the oxygen is united to the copper with which it was in contact in fig. 85, and the atoms of oxygen and hydrogen following from particles of water; the terminating atom of hydrogen uniting with the oxygen of the oxide, the poles of both being reversed. If the particles of water thus formed, being exchanged for atoms of oxygen and hydrogen so as to reproduce the arrangement in fig. 86, which from the high temperature and consequent gaseous state of the water will be readily effected, the current will again take place, but in a reverse direction; the particle of oxide, represented in fig. 86, will be reduced, and again formed and prepared to repeat the operation, as it is represented in fig. 85. The temperature required to continue these operations will of course be preserved by the union of the oxygen with the hydrogen; and the superfluous light will render the metal incandescent by its radiation. It is probable that the metal is reduced by hydrogen, in the absence of oxygen, by circuits formed in the oxygen alone, upon the same principle.

* See Capt. Yorke's experiments in which he discovered the negative character of oxide of lead. Phil. Mag. Vol. V. p. 83.

See also M. de la Rive's Researches, in which it is stated that peroxide of manganese is negative to platina. Phil. Mag. Vol. II. p. 278.

M. Schoenbien lately discovered that peroxide of lead is negative to platina; and that peroxide of silver is negative to peroxide of lead. Phil. Mag. Vol. XII. p. 225.

The remainder of this paper will be chiefly devoted to the application of these principles to the peculiar voltaic condition of iron discovered by Professor Schoenbein. It may not be amiss to state in general terms, that the protection given to iron in all such cases, arises from its presenting to the acid a positive surface, with which oxygen, a body electrically positive to iron, cannot cohesively unite (103).

The protection given to zinc, by amalgamating its surfaces with mercury, arises from the same cause.

170. It must have been observed that in the whole of the preceding experiments, in which a cohesive union is formed, it invariably takes place between a positive surface of the positive element, and a negative surface of the negative element. I have already shown the impossibility of forming a permanent connexion between such elements upon any other principle (102); and my object will now be to apply this principle to the explanation of various other phenomena.

171. If we put a copper plate upon a zinc plate, it has been shown that the surface of the zinc presented to the copper will be positive, and its opposite side negative (49); but if we put the zinc plate in contact with, and between, two similar copper plates, it will present positive surfaces to both, and the corresponding negative surfaces will face each other towards the centre of the plate. It would, perhaps, be difficult to bring copper plates of precisely equal power into action in this manner; but by amalgamating the plate upon the plan discovered by Mr. Sturgeon, we, in effect, do the same thing; we surround the zinc plate with a metal which is relatively negative to it, and thus, agreeably to the laws of electrical action, induce upon it a positive surface. The positive poles of the atoms of the plate being presented to the mercurial coating in all directions, the corresponding negative poles must be directed towards the interior of the plate. There is no development of these poles upon the surface of the plate; because the force which induces a positive polarity is the same upon every part of this surface.

172. In this arrangement we have a double protection. We have, in the first place, the protection of an uniform surface, upon which there can be no inducement to form local circuits; and, in the next place, we have an uniformly positive surface, between which and oxygen, or an other positive element, there can be no chemical union (103). If, however, the amalgamated plate be brought into contact with a metal of higher negative power than mercury, such as copper or platina, these conditions are removed; the zinc becomes more highly positive at the points of contact, and a corresponding

negative surface is developed on some other part of the plate. Circuits are now formed by the surrounding fluid, from the negative surface developed upon the zinc, to a corresponding positive surface developed at the same time upon the negative metal; and the oxidation of the zinc proceeds at its negative surface, with the usual current through the fluid to the positive surface of the negative metal, where the negative elements are deposited. The same effect is produced when the amalgamated plate forms part of the arrangement of a voltaic battery; but the instant the circuit is broken, in either of these cases the amalgamated zinc plate presents its uniformly positive surface, and chemical action is again suspended.

173. We have now arrived at a point in the enquiry from which we may easily extend our views to the peculiar voltaic condition of iron discovered by Professor Schoenbein. The following is an abreviated account of this discovery.* "If one of the ends of an iron wire be made red-hot, and after cooling be immersed in nitric acid, sp. gr. 1.35, neither the end in question nor any other part of the wire will be affected, whilst the acid of the said strength is well known to act rather violently upon common iron." "By immersing an iron wire in nitric acid of sp. gr. 1.5 it becomes likewise indifferent to the same acid of 1.35." "Any number of iron wires may be made indifferent to nitric acid by the following means:—an iron wire with one of its ends oxidized is made to touch another common iron wire; both are then introduced into nitric acid of sp. gr. 1.35, so as to immerse the oxidized end of the one wire first into the fluid, and to have part of both wires above the level of the acid. Under these circumstances no chemical action upon the wires will take place, for the second wire is, of course, but a continuation of that provided with an oxidized end. But no action occurs, even after the wires have been separated from each other. If the second wire, having become indifferent, be now taken out of the acid, and made to touch at any of its parts not having been immersed, a third wire, and both again introduced into the acid so as to make that part of the second wire which had previously been in the fluid enter first, neither of the wires will be acted upon either during their contact or after their separation." "A wire made indifferent by any of the means before mentioned is immersed in nitric acid sp. gr. 1.35, so as to have a considerable part of it remaining out of the fluid; another common wire is put into the same acid, likewise hav-

* Philosophical Magazine, Vol. IX. p. 53.

ing one of its ends rising above the level of the fluid. The part immersed of this wire will, of course, be acted upon in a lively manner. If the ends of the wires which are out of the acid be now made to touch one another, the indifferent wire will instantly be turned into an active one." Any metal connecting the two ends of the wires in question produces the same effect as their direct contact. "A platina wire was connected with (what we call*) the negative pole of the pile, an iron wire with the positive one. The free end of the platina wire was first plunged into nitric acid of sp. gr. 1.35, and by the free end of the iron wire the circuit closed. Under these circumstances the iron was not in the least affected by the acid; and it remained indifferent to it even after it had ceased to perform the function of the positive electrode."

During the inactive state of the wire the oxygen arising from the decomposition of the water was evolved at the anode in the same manner as if the electrode had been platina. But "if the evolution of oxygen at the anode be ever so rapidly going on, and the iron wire made to touch the negative electrode within the acid, the disengagement of oxygen is discontinued, not only during the time of contact of the wires, but after the electrodes have been separated from each other."

It is an indispensable condition for causing the evolution of oxygen at the iron wire to close the circuit in the manner before mentioned; for if the circuit be closed with the platina wire or by first plunging one end of the iron wire into the nitric acid, and by afterwards putting its other end in connexion with the positive (negative) pole of the pile, a nitrate of iron is formed, even in an acid containing 400 times its volume of water.

174. It will be readily perceived that all the experiments which accompany this discovery are consistent with the supposition that iron wire when in the peculiar state presents to the acid a positive surface; and that this is the case we shall be able to show in a sufficient number of instances to render it probable that this is the foundation of the general principle upon which the iron is protected.

In the explanation of Dr. W. C. Henry's experiments (169), we arrived at the conclusion that the oxide of copper is negative to copper: and in the experiments of Captain Yorke, to which we then referred, it was proved that although pure lead is positive to iron, when oxidized it becomes negative both to

* What I call the positive pole (62).

iron and copper.* We have consequently some ground for assuming that the oxide produced by heating the wire in the first experiment, was negative to the iron; and that it therefore induced upon the wire a positive surface; upon the same principle that mercury produces a positive surface upon zinc (170). Again, we found upon investigating the cause of the additional electro-motive force derived from the addition of nitric acid to the electrolyte, that the acid when deprived of part of its oxygen, left nitrous acid or some other oxide of azote in a state highly negative at the cathode, instead of hydrogen (90. 158). Now, it appears very probable that when iron is plunged into strong nitric acid, part of its oxygen combines with the iron, leaving this negative principle adhering to its surface; which, in accordance with the laws of electrical action, will induce upon this surface a positive polarity.

175. If we admit that the ends of the wires thus prepared are rendered positive, our previous observations will account for their inactivity (170). But in every other case to which our attention has been directed, the surfaces have resumed their natural state upon the removal of the deranging force; whereas in the present instance, the wire not only retains its peculiar condition when removed from the acid, but is capable of communicating it to other wires with which it is associated; the state, therefore, to which the iron is brought in these experiments, is in some degree fixed in the wire: that is, it is so far fixed as to retain its state until a greater force of an opposite character is brought to act upon it.

176. Now, the only state of derangement that we can communicate to bodies which are capable of being fixed, and then propagated to other bodies, is the magnetic state. In the present case we have a metal known to be highly susceptible of magnetism; and which only requires some foreign body of a different capacity for light to be immoveably associated with it, in order to fix its atoms in the state of derangement which is communicated to them. In the common artificial magnet, this body is found in the carbon by which the iron is converted to steel; and it is rendered immoveable by hardening. In the experiments before us, when the peculiar condition is communicated by heat, the foreign body is found in the oxide which is fixed on the surface of the wire; and when the wire is plunged into strong nitric acid, it is found in a film of the acid deprived of its oxygen by the wire or in

* By a communication received from M. Schoenbein since the above was written, we learn that peroxide of lead is negative to platina, and that peroxide of silver is negative to both.

the oxide formed by these means. A single section of atoms in possession of a different electrical force, is fully sufficient to communicate, to extend, and to retain the derangement; more particularly in a body of such narrow dimensions as a wire.

177. It is necessary to observe, that a body, any part of which is in a state of derangement, extends it, from atom to atom, to other parts of the body and to other bodies in succession, until the force is exhausted; and the derangement thus extended is precisely similar to the original, whatever may be its character.* When, therefore, the positive polarity, which constitutes its peculiar condition, was communicated to the end of the wire, in these experiments, it was immediately propagated from the end to every part of the wire, and to any other wires brought into contact with it. But the derangement communicated to unprepared wires by induction, will only continue while the inductive or deranging force is in action, unless it be fixed by some other means previously to its removal; the common wire, therefore, while under the influence of the peculiar wire, is plunged into nitric acid sp. gr. 1.35, which appears to have the power of fixing the derangement, though it has not the power to communicate it.

178. It is necessary, however, that the state of derangement should be communicated to the common wire by being in contact with the prepared wire, previously to its being introduced into the acid; for when the end of a common wire is plunged into nitric acid sp. gr. 1.35, there is, of course, the usual action, which requires the local circuits we have before described between positive and negative surfaces, and, consequently, the development of both: when therefore the end of an inactive wire is also plunged into the same acid, not being in contact, and the two ends of the wires not immersed are made to touch each other, each of the wires will endeavour to communicate to the other its own state, and, of course, that which has the greatest force will prevail. We find by experiment that in this case the active wire is the most powerful, the indifferent wire being rendered active.

179. When iron was made what is usually, but I believe erroneously, called the positive and platina the negative electrodes in a voltaic battery, the platina wire being first plunged into the nitric acid (sp. gr. 1.35) which formed the electrolyte, and the circuit closed by the free end of the iron wire, the latter became passive, but in any other mode of closing the circuit it was rendered active. This experiment,

* See abstract (14).

in which the active or passive state of the wire is determined by the priority of its connexion with the acid or the zinc, in closing the circuit, presents, at first view, some little difficulty; but if we recollect that the surface of the zinc to which the wire is presented is negative, its positive surface being in contact with the copper, we shall perceive that the zinc is prepared to act as platina, by inducing a positive surface on the part of the wire which is presented to it; and this will bring it into the numerous class of experiments in which the peculiar state is communicated to the wire in consequence of its being associated with platina. In any other method of closing the circuit, the wire is thrown into action as any other unconnected iron wire would be when plunged into acid of the same strength; and the negative state of the surface of the wire essential to this action, concurring with the inductive influence of the zinc as it is exercised under ordinary circumstances, the action is continued when the circuit is completed. The positive state of the wire when in its peculiar state, is proved by its being thrown into action when touched with what is, improperly, called the negative electrode; the positive surface of which, agreeably to the ordinary course of electric action, induces upon the iron wire a negative surface.

The platina acting as the positive electrode, produces an opposite effect to that which follows from its contact when unconnected (152); in the latter case the effect arises from the negative character of the metal; in the former, therefore, it is reasonable to conclude that the change which has been induced in it by its connexion with the battery, has given it an opposite, or a positive, character. Many instances of this twofold character of platina, as well as other metals, [illegible] on its position in the voltaic arrangement, have [illegible] the progress of this enquiry (116 [illegible]). When [illegible] both the anode and the cathode are of platina, the [illegible] force must be derived entirely from the polarity [illegible]d to them by the battery: it cannot be derived [illegible] rent, because no current can take place unless the arrangement be previously formed; the positive [illegible] refore, in making this arrangement, must be directed to the anode, and the negative elements to the cathode, by polar forces communicated to the platina electrodes by the battery (168).

180. The great and, indeed, the only peculiarity in the means by which an iron wire acquires its inactive state, is that the positive poles of its atoms are all turned towards the surface of the wire, and the whole of its negative poles towards the interior of the wire; the arrangement being, in every

respect similar to the atoms of a zinc plate when amalgamated (171). This arrangement is altogether different to that usually produced either by electric or magnetic induction; the opposite polarity being developed by both these agents, upon some other part of the surface, in every other instance with which we are acquainted. In the experiments in which one of the ends of the wire was oxidized, either by heat or nitric acid, the positive polarity of the surface was readily accounted for; this state once fixed, and surrounding any part of the wire, being necessarily propagated over the whole of its surface; but when the peculiar condition is given by putting the wire in communication with the negative surface of the zinc of a battery, by making it the electrode at the anode, as in the last experiment, or when it is given by touching it with platina when both are immersed in nitric acid, it does not readily appear why the positive surfaces induced by these means, should be propagated from atom to atom round the wire, the negative surfaces being turned towards its centre, instead of being propagated by alternate positive and negative surfaces upon the atoms in the direction of the length of the wire in the usual manner. Taking this peculiar mode of propagation as we find it, every experiment brought forward to illustrate this highly interesting subject, will be readily explained, upon the principles we have advanced, by the state of the wire which must necessarily result from it.

181. In a letter which accompanies M. Schoenbein's communication, Dr. Faraday has given some experiments, of which the following is a short abstract.*

"If a piece of ordinary iron wire be plunged wholly or in part into nitric acid of about specific gravity 1.3 or 1.35, and after action has commenced it be touched by a piece of platina wire also dipping into the acid, the action between the acid and the iron wire is instantly stopped. The immersed portion of the iron becomes quite bright, and remains so, and is in fact in the same state, and can be used in the same manner as the iron rendered inactive by the means already described." "This effect is the more striking if it be contrasted with that produced by zinc; for the latter metal, instead of protecting the iron, throws it into violent action."

Charcoal and plumbago, like platina and gold, have the power of bringing iron into the inactive state. When steel was immersed in nitric acid, there was at first action of the usual kind, which, being followed by the appearance of the well known black carbonaceous crust, the action immediately

* Philosophical Magazine, Vol. IX. p. 57.

ceased, the steel being protected by association with the carbon evolved.

182. The whole of these experiments are evidently dependant upon the principles I have already explained. When the iron wire is touched with platina or charcoal, both negative bodies, they induce upon the wire a positive surface; and this, propagated in the peculiar manner (180), protects the wire. When, on the other hand, the iron wire is touched with zinc, a positive body, it induces a negative surface and the wire becomes active. Dr. Faraday remarks that these states are well observed by putting the iron wire into nitric acid of the given strength, and touching it in the acid alternately by pieces of zinc and platina; when it becomes active or inactive accordingly as it is touched with one or the other.

183. The reasoning and experiments of Dr. Faraday render it probable that, whatever may be the means employed to produce it, the peculiar state is induced and fixed in the wire by a superficial film of oxide, which, from its peculiar condition, the acid cannot dissolve. This film of oxide appears to have the power, not only of inducing the positive state essential to the inactivity of iron, but also of *fixing* the polar surfaces of its atoms in the position in which they are produced by its own negative force or by the inductive influence of other negative bodies; and the positive polarity being *fixed* upon any part of the surface of the wire, its propagation over the whole of its surface, unless it be opposed by a superior force, becomes a necessary consequence; the derangement propagated being in every case precisely of the same character as the original. I am the more confirmed in this opinion by the current from the iron which usually precedes its becoming inactive, this being a necessary attendant on oxidation with decomposition.

184. The extension of derangement is beautifully displayed in a later experiment of Professor Schoenbein.* An inactive wire was wetted by a solution of sulphate of copper and then touched, on any point of the part wetted, with a piece of common iron, zinc, cadmium, tin, lead, arsenic, or even copper, when the precipitation of copper instantaneously took place at the point of the iron wire where contact had been effected, and this action rapidly extended itself over the whole part of the wire which was covered with the solution. Here the inductive influence of the positive metal produced upon the iron a negative surface at the point of contact, being the reverse of its previous state; and this was extended

* Philosophical Magazine, Vol. IX, p. 269.

to the whole surface of the wire, though only observed on the part of it which was covered with the solution.

185. The magnetic character of the derangement is strikingly illustrated by an experiment which precedes the last, in which an inactive wire was rendered active by making it vibrate. The poles of the atoms were in this case reversed by the very means which have been known to produce the same effect on magnets; though, more generally, vibration has a tendency to produce neutrality. This, indeed, may have been the case in the present instance; for the wire only required to be brought into its ordinary state to become active.

186. I shall conclude this subject by giving a short account of a more recent communication of M. Schoenbein;* in which the peculiar voltaic condition of iron was excited by peroxide of lead. I introduce it, not only because it is interesting in itself, but also because its explanation will bring into view a curious display of electrical action.

"The most powerful voltaic association into which iron can be brought in order to excite its peculiar condition, is that with peroxide of lead. A common iron wire, one of the ends of which is covered with this substance, proves to be inactive, not only towards nitric acid of a given strength, but towards nitric acid containing any quantity of water; whilst an oxidized iron wire, or one associated with platina, &c., is acted upon by that acid, if much diluted, just in the same manner as unprotected iron."

"By the relation of iron associated with peroxide of lead to nitric acid and to the solution of blue vitriol, as well as by the fact that iron in this combined state excites a very strong taste upon the tongue, I was led to suppose that a powerful battery might be constructed of pairs consisting of iron and the said peroxide. Experiments have proved the correctness of my supposition: an iron wire 0''.5 thick, 3'' of length, one of its ends coated with a thin film of peroxide, and each end put into a separate vessel filled with nitric acid a hundred times diluted, developed a current which was capable of decomposing water. For when the two vessels were connected with a platina wire, hydrogen was evolved at one of its ends, oxygen at the other. The latter gas was disengaged at the extremity of the platina wire which was placed in the vessel where the peroxide of lead was. Twenty-four such little wires arranged as a *couronne des tasses* and the before-mentioned diluted nitric acid used as the exciting liquid caused a current of con-

* Philosophical Magazine, Vol. X. p. 425, 428.

siderable intensity; for it rapidly decomposed water only slightly acidulated and produced likewise a sensible shock. But, as may easily be foreseen, such a pile is not active for a great length of time; for the hydrogen evolved at the negative (positive) part of each wire, that is to say, at the end covered with peroxide of lead, rapidly decomposes this substance, thereby reducing the wire to its ordinary state."

187. In these experiments the positive surface is induced upon the iron wire by coating one of its ends with peroxide of lead; a substance which Captain Yorke experimentally discovered to be negative to iron (174). But the peroxide is reduced by giving up its oxygen to the hydrogen of the decomposed water; and in its metallic state it is positive to iron.* Upon its acquiring this state, therefore, the poles of the wire are reversed, and the iron becomes active. The current is from the uncoated end of the iron wire, with decomposition of the water, and oxidation of the wire, to the platina wire, where hydrogen is evolved; it then passes through the platina, and upon entering the second vessel the water is again decomposed, the oxygen being evolved at the surface of the platina, and the hydrogen, instead of being separated as before, united with the oxygen of the peroxide and reconverted to water.

188. From two communications made by M. Schoenbein since the preceding explanation was written, we learn several other curious particulars respecting the voltaic relations of certain peroxides, platina and inactive iron.† He states in the first of these communications that he found by means of a very delicate galvanometer, "that iron being in its peculiar condition and associated with platina gives rise to a sensible current if put into nitric acid, be the latter ever so strong or somewhat diluted with water." The current excited under the circumstances mentioned is not a momentary, but a continuous one, and at the same time quite independent of the oxidation of the iron. The direction of the current in question is such as it would be if the latter metal was attacked by the acid, that is to say, inactive iron is positive to platina." In the second communication he says, "On a mixture of iodide of potassium and gelatinous starch being put in connexion with the electrodes, signs of decomposition are certainly ex-

* It appears from a later communication of M. Schoenbein, that the peroxide is reduced by these means to protoxide and then dissolved by the acid (190); this will produce the same effect by leaving the wire in its natural state.

† Philosophical Magazine, Vol. XII. p. 225, 311.

hibited, a very small coloured spot making its appearance round the positive electrode, which spot, however, does not sensibly increase in size or intensity of colour, however long the action of the pile may last." "Although there is apparently no chemical action going on in the pile described, still such action takes place at those parts of the iron wires which are placed immediately above the acid. That such is really the case appears from the fact, that after some time the parts mentioned are losing their metallic lustre and covering themselves with a brownish film." "Some might, perhaps, be inclined to think the whole current of my iron platina pile produced by that slow oxidation of the iron wires which takes place at and above the level of the acid, but by most conclusive experiments I have ascertained that such is not the case."

189. The arguments and experiments which follow do not appear to me to be by any means conclusive on this point; and I am of opinion that the oxidation of the iron, already described, is sufficient to account for the current that takes place, which, though continuous, is "extremely feeble, being only indicated by most delicate galvanometers." I am the more decided in this opinion, because I can trace the current to chemical action in every other experiment which M. Schoenbein has brought forward in support of his hypothesis, that current electricity may be excited by chemical tendencies unconnected with any actual chemical results.

190. The following is a continuation of the extracts already given from the first communication. "Another fact, as curious and interesting as that just stated, is the following one. Two platina wires being connected by one set of their ends with the galvanometer, and by the other set with nitric acid or an aqueous solution of sulphate of copper, excite a current, provided one of the ends (immersed in the fluid) of one of the platina wires be covered with a film of peroxide of lead. The current passes from the platina through the fluid to the peroxide: when the said film is so thin as to produce what are called 'Nobili's colours,' it disappears within a very few seconds after having been immersed in nitric acid, and the whole arrangement connected with the galvanometer. From the facts stated it appears that platina is positive with regard to peroxide of lead, and that the disappearance of that compound is caused by a current which eliminates hydrogen at the negative peroxide, by which means the latter is reduced to protoxide of lead and rendered soluble in nitric acid. In a similar manner I have ascertained that the voltaic relation of inactive iron to peroxide of lead is exactly the same as that

of platina to the said peroxide. In using peroxide of silver instead of that of lead, voltaic effects are produced quite the same as those which were just spoken of, that is to say, a continuous current is excited, to which the peroxide acts the part of the cathode, and either of the metals in question, that of the anode. As to the voltaic relation which one of the peroxides mentioned bears to the other, my experiments have shown that peroxide of silver is always negative with regard to the peroxide of lead, be the fluid made use of nitric acid or a solution of blue vitriol."

191. In the second communication he says, "Before concluding my letter, I must not omit to say some few words about the decomposition which peroxide of lead undergoes when voltaically associated with platina and put either into nitric acid or a solution of blue vitriol. In the first case nitrate of lead is formed, in the second sulphate of lead. The said peroxide and platina being substances, each separately quite indifferent to any of the fluids named in a chemical point of view, how are we to account for the deoxidation of the peroxide? No doubt can be entertained that in the circumstances mentioned a decomposition of water takes place, and that the hydrogen of the latter unites with one of the two equivalents of the oxygen contained in the peroxide." But by what means is water decomposed?

192. I shall answer this question in Professor Schoenbein's own words, placing between parenthesis the parts of the explanation which I conceive are introduced without the support of sufficient evidence, and which to render the explanation correct ought to be omitted. "According to my opinion, there is only one way left to account for the very remarkable decomposition which peroxide of lead undergoes in the above-mentioned circumstances. By putting a voltaic association of peroxide and platina into nitric acid, a current (of tendency) is excited, which, as already stated, passes from platina through the fluid to the peroxide. This current (though it be too weak to cause any electrolization, nevertheless) acts upon the particles of water placed between the electrodes, in such a manner as to turn their hydrogen side towards the cathode, the oxygen side towards the anode, and to diminish or destroy the chemical attraction mutually exerted by the oxygen and hydrogen of each particle of water. Now if we admit of such a state of things, it is not difficult to conceive how the hydrogen of that particle of water which is contiguous to a particle of peroxide of lead can combine with one portion of the oxygen of the latter, the peroxide. (The second equivalent of oxygen of this substance, being of itself rather loosely

united to lead, and having its affinity for the said metal still more weakened by the tendency of the acid to unite with the protoxide of lead, we may consider the second equivalent of oxygen as almost free, and, consequently, as endowed with a great affinity for hydrogen. Being in such a condition it can easily unite with the particle of water which is in immediate contact with the peroxide, and the combination of the two elements will take place the more readily, that the hydrogen is, from the reasons above stated, likewise in almost a single state.) The oxygen set free from the said particle of water must (from obvious grounds) combine with the hydrogen of the second particle, the oxygen of the latter with the hydrogen of the third, and so on, until the whole row of particles of water placed between the electrodes have, as in common electrolization, undergone a similar decomposition and recomposition. Oxygen must, of course, be at last disengaged at the positive electrode."

193. With the omissions pointed out, and other trifling exceptions, this, as far as it goes, is precisely the explanation of the experiment which I should have given if the second communication which contains it had not made its appearance (68).* But I do not see in this explanation the "obvious grounds" upon which the several decompositions and recompositions take place, nor do I think the author himself could have been aware of them; for immediately preceding this quotation he says:—"one is really at a loss to imagine any reason why the hydrogen of water should tend to unite with the oxygen of the peroxide, in order to form water again. The affinity of hydrogen for oxygen being satisfied, it would be, if I am allowed to speak in a metaphorical manner, on the part of the former, a most wanton capriciousness and groundless love of change, should it try to leave one particle of oxygen to combine with another one of precisely the same kind."—I shall now, therefore, endeavour to supply this deficiency, to render the explanation complete.

194. The two platina wires being connected with one set of their ends, may be considered as forming the same body of platina; the peroxide of lead at one end of this body, being negative to platina, will induce upon it a positive surface, and this, propagated through the wire in the usual manner, will terminate at the other end in a negative surface. To this surface, forming the anode, the oxygen sides of the particles of water will be directed; and to the peroxide of lead, forming

* In the reference here made, Vol. II. p. 369, line 11, for *to* the last atom, read *of* the last atom.

the cathode, the hydrogen sides; not by the force of the current, as stated by M. Schoenbein, but agreeably to their respective electrical states in relation to positive and negative surfaces (64). A transfer of light having been made from the peroxide, the negative body, to the platina, the positive body, when first brought into contact with it, upon completing the circuit with the electrolytic fluid, which will be arranged in the manner we have described, there will be a current from the latter, the platina, to the negative surfaces of the atoms of oxygen, which are turned towards it, from the positive surfaces of these atoms to the negative surfaces of the atoms of hydrogen connected with them, from the positive surfaces of these atoms to the negative surfaces of the next adjoining atoms of oxygen, from the positive surfaces of these atoms to the negative surfaces of the atoms of hydrogen connected with them, and so on in succession, until the poles of the whole of the atoms which form the rows of particles of water placed between the electrodes are reversed; when the decomposition and recompositions described by M. Schoenbein will necessarily take place (69, 102). In this combination the peroxide of lead takes the part of the copper, and the platina the part of the zinc in common voltaic arrangements. The chemical action which usually takes place by the oxidation of the metal at the anode is here effected by the deoxidation of the peroxide of lead at the cathode, and the current is rendered continuous in the two cases upon the same principle, by chemical operations the reverse of each other; the circuit with which each atom is independently connected (118), being broken in the former upon the oxidation of the zinc, and in the latter upon the deoxidation of a particle of the peroxide. A new arrangement in the electrolyte is required in both cases, and in the mean time the metals return to the intermediate state (58), which prepares them to repeat the operation when the circuit is again completed.

125. If chemical action be the mechanical means which, by alternately breaking and completing the galvanic circuits in rapid succession, renders the current continuous, something analogous to it may be expected from human ingenuity; and it so happens, that we have a little philosophical apparatus which is kept in action upon a similar principle. De Luc's electric column, it is well known, is usually formed of a great number of alternations of zinc foil and thin paper covered with silver leaf, so arranged, that the order of succession may be zinc, silver, paper, zinc, silver, paper, &c. throughout. The electric column consists of about five hundred such alternations inclosed in a glass tube, surmounted at the ends with brass caps,

which serve as the poles of the arrangement; the cap at one end being in communication with the zinc foil and that at the other end with the silvered paper. A variety of amusing experiments are performed by means of this column, one of which consists in giving motion to the gold leaves of an electrometer, which for this purpose must be separately insulated, and placed at a little distance from each other.

If we connect the zinc end of the column with one of the gold leaves of an instrument thus constructed, and the silver end with the other, they will immediately attract each other, and upon coming in contact the circuit will be completed, and a current will pass from the zinc to the silver end of the column, which will cease when the different surfaces are in a state of equilibrium with the new forces thus brought into action (56. 57); the attraction between the gold leaves will cease at the same time, and they will separate by their own weight. But the moment the gold leaves separate, the circuit being broken, the column will return to the state in which it was previously to their coming in contact, or to what I have called the intermediate state, and the different actions will be repeated with similar results. The galvanic current in this case is perfectly independent of chemical action (59), and the apparatus is, *in principle*, a true perpetual motion: the tarnishing or slight oxidation of the metals, which necessarily take place, so far from assisting its operations, being, in fact, the principal cause of its loss of activity and ultimate failure.

196. It is remarkable that M. Schoenbein, immediately after stating that no electrolyzation takes place in his experiments, should proceed to describe what I conceive to be the true electrolytic arrangement. I am not aware that this arrangement has before appeared except in publications of which I have myself been the author. The arrangement in Dr. Roget's theory,* which I believe has hitherto been almost generally adopted, is very different to this; it supposes, if I understand it correctly, that the hydrogen and oxygen sides of the particles of water, when placed in electrolytic order, form two parallel rows (as in figure 87†), that while the former are moving together, by the agency of electricity, in a direction towards the negative metal, the latter are moving together in the opposite direction towards the positive metal; and, conse-

* See Library of Useful Knowledge; article, Galvanism, No. 106.

† In the figure, the circuit being supposed to be completed, one of the atoms of hydrogen is advanced to a state of separation at the surface of the copper plate, and one of the atoms of oxygen to a corresponding state at the surface of the zinc plate.

quently, the atoms of the former will merely have to pass over or under the atoms of the latter, depending upon whether the hydrogen or the oxygen be uppermost, until they are separated in succession at the surfaces of the metals opposite each other. If it were possible that such an arrangement could take place, the water would be a conductor from the zinc to the copper without decomposition (67, 105.); but the arrangement and the operations consequent upon it are altogether without a motive, and, with the exception of the evolution of the two elements in their proper places, wholly inconsistent with the phenomena exhibited by the experiment.

It appears by the following quotation from the abstract of one of Dr. Faraday's Series of Researches, that his views on this subject are somewhat similar to those of Dr. Roget. "The peculiar feature of this mode of discharge, however (that which attends electrolyzation), is in its consisting, not in a mere interchange of electric forces at the adjacent poles of contiguous particles, but in their actual separation into their two constituent particles; those of each kind travelling onwards in contrary directions, and retaining the whole amount of the force they had acquired during the previous polarization."

It is highly gratifying to me to record in the same paper two instances in which philosophers so distinguished as Professors Faraday (122) and Schoenbein, have adopted opinions which, though different to those usually received, have long formed the leading principles of my theory.*

* Two of my papers were read before the Royal Society in June, 1835, entitled, "Memoranda relating to a theory of sound," and "A Theory of the Tides, including a theory of the formation and propagation of waves." The following abstracts of them, given in the seventh volume of the Philosophical Magazine, will show that the principle claimed as a discovery by Dr. Faraday in 1838, was one of the leading principles upon which the explanation of those phenomena was then attempted.

"The author advances the hypothesis that each particle of an elastic body, after receiving an impulse in a particular direction, and communicating that impulse to the adjoining particles, instead of being thereby brought to a state of rest, is carried back by its elasticity with a velocity which continues its motion beyond the point from which it originally set out, and is thrown into continual vibration, in a manner analogous to the motion of a pendulum. He endeavours on the principle of a continual transfer of the state of each particle to the adjacent particles, to explain the phenomena of continued sound arising from a prolonged succession of vibrations." "The author applies the same principle announced in his

197. Much interest has been excited by the discovery of the protecting influence of zinc upon copper, when exposed to the action of sea water: but, although the principle upon which this protection takes place is well known, I believe the precise means by which the object is accomplished have not yet been described. I have already endeavoured to explain some experiments of Dr. Faraday's, in which a current was produced through two parallel plates of zinc and platina, with decomposition of water at one end, iodide of potassium, both

paper on the theory of sound, namely, that of a continual transfer of state, between the adjacent atoms of a medium, to the case of oscillating columns of fluid constituting waves and tides."

In 1836, I published the abstract of a series of papers entitled, "Outlines of a theory intended to connect the operations of nature upon general principles." From which the following are extracts. After speaking of the communication of electricity by induction, it says, "14. The great principle of derangement, whatever may be its character, is, that it is extended to other bodies, with all its circumstances, precisely as it exists in the original; and this extension is made through all bodies, indiscriminately (*though they differ in conducting power*), from atom to atom, until the force is exhausted. This extension of derangement is usually called induction." This passage is quoted in a paper in the Annals, vol. 1, page 234, with the addition here introduced in italics. "27. Like other kinds of derangement, magnetism is extended to other bodies indiscriminately, and the power of attraction which has been supposed to be exercised by magnetic bodies at a distance from each other, is, in fact, exercised by the intermediate body, which becomes a temporary magnet. The facility with which such bodies return to their natural state upon the removal of the deranging force, has prevented this species of magnetism from being observed, except in bodies in a state of rapid rotary motion."

The following extracts from a paper "On capillary attraction and on the disposition there is in fluids to assume a globular form," published in the 4th vol. of the Records of General Science, in 1836, will show that the propagation of polar forces by induction, *in curved lines*, did not escape my attention. In consequence of the great increase of force derived from the completion of the polar circuit, bodies in a state of derangement are constantly disposed to produce the necessary communication between their opposite surfaces, through the medium of intermediate bodies, although they may be very inferior conductors. The magnetic curves formed in the air, and rendered visible by means of iron filings, are of this description." "These *curves*, or rather curves formed upon this principle, which can only give motion to the solid needle as a whole, acting in the same manner, produce the actual union of a line of fluid particles, by bending the line so that the two ends may meet," and form a circle.

placed between them, at the other; and a reference to this explanation and the figures annexed to it will assist me in the present attempt. It will be evident from an inspection of figure 75, and a due consideration of the explanation which accompanies it, (116.) that as it regards the electrolyzation of the water at *y*, the distant part of the zinc plate acts merely as a conductor, and that copper might be substituted for it without lessening the effect; or, in fact, that a copper plate might be substituted for the zinc plate altogether, provided a piece of zinc were placed upon it to receive the drop of dilute sulphuric acid. In this case the current would be from a negative surface of the zinc, placed upon the copper plate at *y*, through the dilute acid, with decomposition of the water, to a positive surface of the platina opposite the zinc; then through the platina to the other end of the plate, where it would pass from a negative surface through the iodide of potassium, with decomposition, to a positive surface of the copper. In this state of the experiment it will be further evident, that, as it relates to the decomposition of the water, the platina plate acts merely as a conductor of the current from the water to the iodide of potassium; and that it might be withdrawn altogether provided its place were supplied by an extension of the dilute acid, so that the hydrogen of the water might be in contact with the iodine. Or the iodide of potassium may be taken away and the dilute acid extended to meet the positive surface of the copper, without any change of effect; the polar state of the surfaces being secured by the contact of the zinc and copper. In this case, the current would be from a negative surface of the zinc through the dilute acid, with decomposition of the water and oxidation of the zinc, to a positive surface of the copper; where hydrogen, corresponding in quantity with the oxygen united to the zinc, would be separated.

198. We have now only to substitute sea water for the dilute acid, to complete our explanation; it being evident that if the electrolyte covered the whole of the copper plate, including the zinc at *y*, circuits would be formed in it, issuing from a negative surface of the zinc and entering into a positive surface of the copper; and this positive surface, as we have already so frequently observed, would be a secure protection from oxidation. In the case of copper protected by zinc, there is a system of circuits formed in the sea water, in curves, similar to magnetic curves, from the zinc protectors to the surrounding copper. If we remove the zinc, the same circuits will be formed; but they will now be local, and the part of the copper which presents the negative polarity will be

corroded, while that part of it which presents the positive polarity will be protected as before. Mr. Sturgeon noticed this effect of local circuits in plates of zinc;* and Professor Schoenbein observed it in some uncleaned iron wires which had been immersed in an acid solution of per nitrate of mercury.†

199. It appears, th ng influence of a positive surface exten ly; and that the peculiar appearances e e chiefly from the fixed character of its is probable that metals which are pro n by gilding and plating, or by varnish this protection to the positive surface which is induced by the negative bodies thus applied, rather than to the actual covering they receive from them; for this covering is generally much thinner than the mercury upon amalgamated zinc, and yet, even in this case, the protection is withdrawn from the zinc when by any means a negative polarity is induced upon its surface.

200. If this inference be corr metals, both liable to oxidation, vided they are sufficiently differ properly arranged for the purpose be covered with a thin film of co the latter metal, when plated, metals would most likely be prot which they would present. By m metal and coating it with a film that tin is applied to copper, or, unite in this way, by placing a th with the copper, it is probable th be given to the copper sheathing per would be exposed as usual to of protection should succeed, it being free from those adhesions to protected by zinc plates; the cir materials in the latter case, bein mer. Perhaps, the usual mode answer the purpose, though not s of the less difference of electrical force between the two metals.

I have seldom ventured to give any theoretical speculations that have not been supported by experiment or facts; but in the present instance, feeling the importance of the subject, I have been induced to do so from a wish that experiments

* Annals of Electricity, &c., Vol. 1, page 19.

† Philosophical Magazine, Vol. 10, page 273.

should be made on a larger scale than any I can command. We have a sufficient proof of the necessity of supporting theory by experiment in the failure of the plan proposed by Sir H. Davy for a similar purpose, which, though founded on correct principles, proved to be defective in practice, from circumstances which, in the state of our knowledge at the time, could not have been developed by theory alone.

*The following references require correction.*

| | | | |
|---|---|---|---|
| Page 302 | line 44, | for (115) | read (116). |
| 305 | .. 20, | for (106) | read (107). |
| 306 | .. 9, | for (104) | read (105). |
| 306 | .. 31, | for (108) | read (109). |
| 312 | . 21, | for (117) | read (118). |

## LXXV. *On Electro-magnetism as a Moving Power. By* Charles G. Page, M. D., *Washington City, D. C.**

After the first successful magnetization of soft iron by the galvanic current, and more especially on the announcement of Prof. Henry's signal experiment, the suggestion naturally occurred to every enquiring mind, cannot this immense attractive power, so easily developed and controlled, be rendered available as a mechanical agent? The first successful step towards the attainment of this object, of which we have any record, was made by Mr. William Sturgeon, a distinguished philosopher of England. The next original invention by which an independent motion was obtained from electro-magnets, was the oscillating apparatus of Prof. Henry, described in a previous No. of this Journal.† The next invention of any note, was that of Dr. Ritchie, now very well known as Ritchie's revolving magnet. This ingenious and simple contrivance will always be regarded as a superb philosophical apparatus. It does not exhibit that astonishing rapidity of rotation, as if its poles were changed by the use of solid conductors, but as an instrument is more pleasing, as it shows at the same time the magnetic rotation, the vivid sparks, and in the dark a beautiful optical illusion. Some time after the announcement of this instrument in this country, Mr. Davenport, of Vermont, published in this Journal a partial description of an electro-magnetic engine of considerable power. It appeared that Mr. Davenport had for a long time been occupied in the subject, and was not aware of what had been previously effected by others. Some time prior also to this period, some interesting

* From Silliman's American Journal.

† Dr. Henry's was the first. See p. 430 of the present vol. Edit.

experiments were described in this Journal, by Dr. Edmondson, of Baltimore, and, indeed, this gentleman appears to have been the first in this country who produced a rotary electro-magnetic machine. Since the announcement of Mr. Davenport's invention, the innumerable experiments which have been performed in this country, in England, on the continent of Europe, and even in the East Indies, have all contributed to prove that the smallest engines which have been made, have had by far the greatest proportionate power. Since I first gave the subject any attention, I have had sixteen different models constructed, each involving distinct principles. From all these experiments the inference is still the same, viz. *the fewer the magnets and the smaller their size, (with certain limits,) the greater the ratio of mechanical power obtained.* Such experience as this appears discouraging, but is by no means sufficient to prove the experiment infeasible. The numerous failures are such as have been incident to the prosecution of all inventions in their early stages. It is much to be regretted, that in our country the invention should be a subject of mercenary speculation, when in reality it has no value except as an experiment, and that the public have been so far misled, as to withdraw that countenance and encouragement which the experiment really merits. We cannot but deplore, that such an interesting branch of science should be so traduced, and that the very name of electro-magnetism should be coupled with empiricism.

There can be no doubt in the mind of any one who may have seen an electro-magnetic engine, that it furnishes a mechanical power already applicable and useful to a certain extent, provided the maintenance of that power be not expensive and difficult. The application of this power cannot be expensive, *if the mechanical or working power of any number of magnets in a machine increase in the direct ratio of the aggregate attractive force;* that this rule does not hold in any of the plans of which, hitherto, we have had any description, I shall prove, when the cause comes to be considered. Yet in certain arrangements this law must obtain, and although the necessary construction be at present somewhat complicated, yet ultimately it doubtless will be simplified. At present, we have no means of computing the extent of magnetization which may be effected by a galvanic pair of given surface, say a single inch, freshly immersed. It must very far exceed that which we ordinarily recognize in our experiments. By great care, I have succeeded in producing an attractive force of over 800 pounds, by a galvanic pair having only ten square inches of zinc exposed; whereas with the usual arrangements, it

required two or three square feet to produce the same power. This power, though so great for the means used, yet probably was not near the maximum procurable from the same extent of surface. It would seem, then, that if the above-mentioned ratio exists in attainable forms of machines, the application of the power cannot be otherwise than cheap. The difficulty of maintaining a uniform power is by no means insurmountable. The faults hitherto have been, the wearing and alloying of the pole-changer and springs, and subsidence of battery action; which are easily demonstrated to be remediable. It is not to be presumed that in the present age, or perhaps ever, we shall arrive at a power from electro-magnetism which shall supplant the steam engine, in its grander operations. Indeed it is not essential that this should be the case, to render the invention even invaluable. Incalculable benefit would be conferred upon society, if a new and simple mechanical power could be procured, available from that of a single man to one or two horses. A multitude of mechanical operations are now carried on by animal or water power, for which a low steam power cannot well be used, from the fact that steam engines below one horse power, are hardly worth the making, and are troublesome and expensive. A very natural question here arises; if such a one horse power can be obtained by electro-magnetism, why cannot two horse, or any extent of power, be made? Theoretically considered, it can be; and electro-magnetic powers can only be limited by the means used. But practically we have already been taught, that (unlike other powers, where the largest engines are the most simple and least expensive) electro-magnetic engines above a certain limit, increase in complication and expense in a much greater ratio than the power obtained. To ascertain this limit, the precise point where economy ceases, is now the great, and ought to be the only, object of research.

There seems to be little doubt, from the data we already possess, that a power equivalent to one horse may be obtained with economy. Before proceeding to point out the obstacles in the way of the application of this power, the following general rules are offered as deduced from actual experiment.

*First.*—Whatever be the rate of passage of the galvanic current, the full magnetization of a bar of iron requires time in proportion to its hardness and *size.* Mr. Wheatstone has calculated the rate of electro-motion in good conductors, to be 188,000 miles in a second. Admitting that electricity, even in its lowest state of tension, passed at this rate, still the time required in giving a very large magnet its maximum charge, would be a perceptible item. Therefore a single impulse or discharge, as from a common electric battery, (be they

quantity, are all greatly (or entirely) neutralized. The secondary consequences of this law are; first, small magnets answer better than large; second, changes of poles, to produce motion, must be suspended with[illegible] the introduction of repulsive powers be not more than sufficient to compensate for the loss; third, the power of a machine does not increase with its velocity.

The second general rule is, that integrity of the conducting and magnetizing wires is of the utmost consequence. By integrity, I mean not only the absence of flaws, fractures, and imperfectly soldered joints, but a perfect molecular arrangement. Bending or twisting a wire impairs its conducting power; and a wire which has once been wound upon a magnet is not fit for the same purpose again.

*Third.*—It is well known that the repulsive power is not equal to the attractive, of the same magnet, be it even of the hardest steel. The difference between the two forces is still greater in electro-magnets, and for the same reason. There is also another cause which operates to diminish the repulsive forces of electro-magnets, which will be considered when treating of the influence of secondary currents.

*Fourth.*—Two electro-magnets, [illegible]
each other, even when similar poles [illegible]
is true of the steel magnets, but not [illegible]

*Fifth.*—Change of poles cannot be [illegible]
for the following reasons:—1. If [illegible]
the same time, the magnets which chang[illegible]
retained somewhat by those which [illegible]
lar poles will attract and produce [illegible]
magnets which change poles be favo[illegible]
of superior conductors, they cannot receive near the same charge as those which do not change; for, first, there is magnetism of an opposite character to be overcome; and secondly, some breaks in the galvanic circuit are necessary to produce change of poles. 2. Two magnets which have a statical repelling power, that is, a power which will merely keep them asunder while the machine is at rest, will attract each other when the machine is in motion. This singular fact is a consequence of secondary currents, shortly to be described.

The next law to be observed is, that the sum of the forces of any number of magnets charged by one battery, is in a diminishing ratio to the forces of one magnet charged by the same battery, provided the battery be not in excess. Hence there must be a great loss of power, when a number of magnets are charged by the same battery. The secondary current has also an important bearing upon this case.

One of the greatest obstacles we have yet to encounter, in the prosecution of this subject, is the influence of secondary currents to diminish the power of a machine, just in proportion to the use of those which at present we consider the most obvious means of increasing the power. By secondary currents are here meant, those currents which flow in the conducting wires, either with or against the battery current, and are consequences of the development or cessation of magnetism, or of the approximation or recession of two charged magnets. These currents are found to obey the following laws.

The battery power remaining the same, the more coils surrounding the magnet, the greater the power of the secondary current.

After one coil has been wound upon a magnet, the addition of a second coil increases the power of the secondary current in a greater ratio than the power of the magnet. Hence, as it has been found, some machines have had greater power with two coils of wire on the magnets than with four or five; although actual experiment proves, that the real or statical power of the magnets is considerably greater when a large number of coils is used. According to Faraday's interesting discoveries, when magnetism is developed in a bar of iron enclosed within a helix, a secondary current flows in the helix contrary to the battery current. When the magnetism ceases, the secondary flows in the same direction as the battery current. The development of magnetism is equivalent to the determination or movement of magnetic forces towards the poles. The cessation of magnetic power is equivalent to the retreating of those forces. Now the approximation of two electro-magnets attracting each other, occasions an additional movement or accumulation towards the poles, and consequently developes a secondary current flowing against the battery current. The power of this current is in proportion to the velocity with which the magnets approach each other.

When two such magnets in proximity or contact are separated by mechanical force, a recession of accumulated forces takes place, and consequently a secondary is developed, flowing in the same direction as the battery current. Therefore, an independent motion of an electro-magnetic machine diminishes the influence of the battery current in proportion to its velocity; whereas the application of mechanical force to drive the machine against its own motion, contributes to the magnetizing power of the battery. The same rule applies to the motion of repelling poles.

When two repelling electro-magnets are made to approach each other, a recession of the magnetic forces takes place, and

consequently a secondary current is developed flowing in the direction of the battery current. While the forces are thus kept in retirement, if the two magnets be made to recede, they will again be determined towards the poles, and consequently the secondary will flow against the battery current. By taking advantage of these laws, I was led to the invention of a new instrument (Magnetic Electric Multiplier,* described in the last number of this Journal,) in which the secondary current may be so applied as to diminish or accelerate the velocity of the revolving bar.

It will now be readily seen, that two electro-magnets, with a statical repelling power sufficient to keep them asunder, would cease to repel when the machine is in motion. The attractive forces constitute the paramount motive power, and when the velocity of the machine exceeds that which the repulsive powers alone would give it, they are of no value whatever, unless they operate in conjunction with attractive forces; but even where this is the case, the secondary current arising from the velocity of the machine, must occasion so great a disparity between the similar poles of the magnets which change and those which do not change, that attraction, in lieu of repulsion, must take place.

I have thus endeavoured to point out the most important of those difficulties in the way of the application of this power, which necessarily arise from the connexion of galvanism and magnetism. There are many other hindrances entirely of a mechanical nature, which perseverance will doubtless overcome.

---

## LXXVI. *Magnetic Electrepeter and Electrotome, to be used with flat spirals.* By CHARLES G. PAGE, M.D., Washington City, D. C.†

Fig. 2, Plate XVI. represents a simple instrument, designed chiefly to aid the operator in exhibiting the magneto-electric properties of flat spirals. Though the flat spiral as a magnetic electrical instrument is inferior to the compound electro-magnet, described in the last number of this Journal, yet the phenomena are more interesting, as they are strictly magneto-electric, produced without the presence or co-operation of ferruginous bodies. The object of the instrument, as its name (electrotome) implies, is to break the circuit, and as it

* This instrument is described at page 423, and fig. 2, Plate XIII. of the present volume of the Annals.—Edit.

† From Silliman's American Journal, for October, 1838.

accomplishes this by changing the direction of the galvanic current, it is also a self-acting electrepeter. A rotating electro-magnet would effect the same object; but the introduction of an electro-magnet or a coiled wire, in any part of the circuit, would detract from the value of the spiral. *g* is a thin base board of mahogany, which, when the instrument is in use, is to rest upon the spiral coil or the box containing it. At the centre of the base *g* is a pivot sustaining the magnetic bar of steel *c* and its axis, the extremity of which plays freely in the centre of the cross piece *h*. Between the upright pillars are secured two circular pieces of mahogany *a b*, *p n*, to serve as supports for the mercury cells *d* and *e*. The circular box *d* contains two concentric mercury cells, insulated from each other, and connected with the poles of a battery by the separate wires and cups *p n*. The centre of this box is open to admit the shaft of the magnet, as is also the centre of the box *e*. This box is made of two glass cylindrical sections, cemented into a groove of a turned cup or base of wood. It contains two cells for mercury nearly semicircular, and insulated from each other precisely as the cells for the Ritchie magnet. These cells are connected with the extremities of the spiral by the separate wires and cups *a b*. The two wires *i i* are well insulated by a winding of varnished silk, and secured in their positions on the shaft by silk thread. The upper extremities of these wires dip into the concentric cells of *d*, and the lower into the cells of box *e*. The base board is made thin, and the pivot *g* short, to allow the magnet to come as near as possible to the spiral. Place the instrument upon the spiral, make the connexions as above directed, and the magnet immediately commences a rapid rotation by the influence of the spiral. The instrument should always be placed without the centre of the spiral, and in such a manner, that the insulating pieces between the cells of *e* should be in the direction of a radius of the spiral.

## LXXVIII. *On the difference in the Electric Capacity of various bodies.* By M. Peltier.*

I mentioned, in a communication made to the Académie des Sciences, Nov. 23, 1835, that copper and zinc have different powers of receiving and keeping, quiescent, either state of electricity; in another communication on the 14th Dec. following, I said that it was the same with pewter, gold, silver, and platina; and, lastly, in a memoir presented to the

* Translated by Mr. J. H. Lang.

Academy, Jan. 9th, 1837, I gave the detail of the apparatus and experiments, by the assistance of which I was led to discover, some years ago, that *the metals have different capacities for one or other static electricity.* Volta, in 1796, made an analogous experiment, by placing in contact two polished plates, the one zinc and the other silver, collecting by condensers the electric state produced at each contact, he obtained incontestable charges. Volta, under the conviction that there exists an electro-motive force in the contact of two heterogeneous bodies, presented this experiment as the most powerful proof of his hypothesis; he even said that the form of the plates, which were well straightened, renders them at the same time condensers, forgetting that to condense and keep apart the two contrary electricities, the plates must be insulated from each other, which was not the case in this experiment; I have also shown (in 1835) that, when soldered, these plates produce the same result.

The proof, that contact has nothing to do with this effect, is, that if we place the two unvarnished plates in contact by their edges, they show no signs of electricity, the influence of the two connected points not being sufficiently extensive, while the contact of the whole surface, notwithstanding the impossibility of a complete condensation, places them in different states of electricity. I am about to give fresh proofs that contact has nothing to do with it.

I made a glass plate of which one surface was gilded, the other covered with platina, being in metallic contact the whole length of the edge, and varnished throughout; I placed it on a golden collector screwed to the electrometer, placing the platina underneath; I then connected this compound plate with the collector by means of an insulated platina wire; the collector was charged with positive, the compound plate with negative electricity. Instead of using a platina wire I afterwards established the contact by means of an insulated zinc and copper pair, in such a manner that first the zinc touched the collector and then the copper. In the three cases the electrical state of the collector was exactly the same; the zinc and copper going for nothing. I then changed
the gold being underneath upon the col
the three methods of contact I had noth
even when I added Bennet's doubler to
tact has no part in this experiment; for
the compound plate had been made negat
the gold side; this latter would have b
influenced the gold collector, which upon
to change sides and make the platina [illegible]

active, we must put a platina collector in the place of the golden one. I shall go still further, and produce the same action without contact between the neighbouring plates.

On a golden collector I placed a platina condenser, and put them in contact; the golden plate took the positive, the platina, negative electricity; this is what I mentioned in 1835. To increase the effect I used Bennet's doubler, which is a third plate placed on the second. I first took a platina doubler, and established the metallic contact between the collector and condenser; the contact being then broken I raised the latter, and the doubler which was upon it. The condenser having a small charge of free negative electricity, I neutralized it by touching with my finger the doubler, which took, by influence, positive electricity; I then replaced these two plates on the collector and re-established the contact between the collector and condenser, then again raising the latter and touching the doubler I caused it to take, in a similar manner, a fresh quantity of positive electricity. I repeated this six times; after which I raised only the third plate or doubler and touched the first, that it might be charged with positive electricity by the influence of all the free negative electricity of the second plate: lastly, I raised the condenser itself, and the electrometer showed 5°. If, instead of the platina doubler, I were to place a golden one and operate similarly the deviation of the electrometer would be about 10°. It is evident from this experiment that the golden doubler has taken and coerced more positive electricity than the platina one, although placed under the same circumstances. If we arrange these plates inversely, by screwing to the electrometer a platina collector, we must have a golden condenser, and then the maximum effect will be negative and obtained with a platina doubler.

In this experiment the third plate or doubler is never placed in connexion with another plate; when we touch it with the finger the second plate is insulated, and this contact, therefore, serves only to permit its receiving, by influence, all the electricity it can coerce. The golden doubler having taking twice as much positive electricity as the platina one, and the latter, on the contrary, having taken twice as much negative, it is evident these metals have different capacities for collecting and coercing the two static electricities, a power which differs essentially from Volta's electro-motive force, one of influence and not of contact. If we neglect to employ this influence of the heterogeneous metals, if the two plates present are homogeneous, if gold, for example, we can never obtain a static effect by touching the collector with platina, even when we

increase the effect of the condensers by the best appropriated doubler. Every time I have had signs of electricity, they were evidently produced by the friction of the discs on their handles, since they were as often in one direction as the other.

This diversity of electric capacity is summed up in the following manner:—

1. Plates of different metals do not take an equal quantity of the same electricity from a constant source;

2. The proximity of a metal influences the condenser and makes them take more of one electricity than the other;

3. In consequence of this influence of distance, two plate condensers being heterogeneous, are influenced reciprocally by making the one more positive and the other more negative, so that, if we establish the contact between them, the gold takes the positive electricity from the platina, and the platina the negative from the gold: on separating them the influence ceases, the surplus electricity acquired becomes free and produces a static effect.

I shall conclude by a final proof. I insulated a *pile á couronne* of a hundred pairs, each of the glasses being well insulated from the next one; each pair was then made to communicate with an electroscope, sometimes by the zinc, sometimes by the copper, and all gave negative signs; while the acidulated water acting on the pairs, gave positive signs throughout. If the productive cause were in the contact of the metals, the zinc would be positive and the copper negative; the liquid would divide these two states, as is the case in a liquid which is only a conductor, such as I have shown in my communication of the 29th Oct. last.

---

## LXXVIII. *Chemical Analysis of Meteoric Iron, from Claiborne, Clarke, Co., Alabama.* By Charles T. Jackson.*

*Aug.* 5, 1834.—Mr. F. Alger handed me this remarkable mineral, which he had received from Mr. Hubbard, who had obtained the specimen during his travels in Alabama, and thought, from the bright streaks in it, that it might be an ore of silver.

On examining this substance, it soon appeared that it was different from any metallic ore of terrestrial origin, and that it is a very peculiar and remarkable meteorite.

Having surmised its probable origin, I was desirous of seeing the gentleman who brought it from Alabama, and at the

* From Silliman's American Journal.

request of Mr. Alger, Mr. Hubbard called upon me and gave me the following particulars as to its locality.

He found the specimen on the surface of the earth, near Little Creek, in Claiborne, Alabama. The soil at that place is composed of red marl, or clay, and the rocks in place are sandstones, mostly of a gray colour. The mass from which my specimen was broken, was of an irregular triangular shape, rounded at the corners, and was 10 inches long, by 5 or 6 inches in thickness. It was extremely heavy, insomuch that he could not conveniently carry with him the whole mass, and therefore employed a negro to break it with a sledge hammer; which operation proving too difficult for him, Mr. Hubbard took the sledge himself, and with the cutting edge, by many hard blows, he ultimately succeeded in detaching the portion in my possession. It is much to be regretted that he did not bring with him the whole mass, and I desired him to send for the remainder, but have not yet heard from him. He is of opinion, that there are many other similar masses near the spot where this was found; but it is not probable that they abound to the extent imagined. I beg leave, however, to call the attention of travellers to the locality mentioned, where the remainder of the specimen still exists neglected.

*Description of the Specimen.*—It is of an irregular form, rounded upon all the sides excepting on that, where it was fractured, which presents a rough hackly surface, with projecting, bright, silvery streaks, and deep greenish and brown eroded surfaces, from which an exudation of green liquid takes place, on exposing the specimen to moist air.

The rounded surface is coated with a thin layer of the *sub-chloride of iron*, which being removed, the mass is found to consist of *metallic matter*, resembling wrought iron, when the specimen is filed bright. On attempting to break off a fragment, the mass was found to be extremely tough and malleable, so as to require the aid af a file and cutting chisel.

Sp. gr. on three separate fragments from different parts of the mass, 5.750, 6.400, and 6. 500. The whole mass weighs 28 ounces avoirdupois.

Having washed the specimen in distilled water several times, I filed one side of it bright, and left it exposed to the air in my cabinet. In a few days, numerous grass-green drops of liquid began to collect on its surface, and became externally coated with a thin, brown film. This liquid had a slight alkaline astringent taste, but gave no alkaline reaction with tumeric paper or brazil wood solution. A few drops collected in a test tube and diluted with water, gave an *abundant thick*

curdy white precipi
showing the presen
cyanate of potash g
and ammonia gave
iron. Muriate of am
of the exudation, t
ammonia, and the r
indicative of nickel,
of nickel formed in t

Thus the green fl
posed of the hydro
doubtless form from the action of the moisture of the atmosphere upon the metallic chlorides contained in the meteorite.

*Analysis of the mass.*—Several fragments of the specimen having been cut off by means of a steel chisel and hammer, their specific gravities were ascertained, and they were then subjected to analysis.

Specimen 1. A fragment weighing 25 grains, sp. gr. = 5.750, being placed in a green glass flask, and pure nitric acid poured upon it, no action took place until heat was applied, when a violent effervescence, with extrication of nitrous acid fumes, began, and the solution was rapidly and entirely effected. The solution was then treated with a sufficient quantity of the solution of muriate of ammonia, to prevent the precipitation of the nickel, and then the peroxide of iron was thrown down by means of liquid ammonia. When the precipitate had subsided, the whole was thrown on a filter, and the peroxide of iron was thoroughly washed, dried, ignited in platina capsule, and weighed = 23.5 grs. peroxide of iron = 16.296 grs. metallic iron.

The solution, which had passed the filter, was of a clear blue colour, with a slight amethystine tint, indicative of nickel. This solution and the mingled washings were evaporated in a glass vessel to a small bulk, and then treated, while warm, with a hot solution of pure potash, when a dense bulky green precipitate of the hydrate of nickel was thrown down, which being collected on a filter, washed, thoroughly dried, and ignited in a platina crucible, weighed 8.8 grains = oxide of nickel = 6.927 grains metallic nickel.

*Analysis*—2d specimen. A fragment of the meteorite weighing 30 grains, was found to have a sp. gr. = 6.500. It was placed in a green glass flask,—perfectly pure nitric acid was poured upon it, and heat was gradually applied until the solution was completed. It was then diluted with pure distilled water, and a solution of nitrate of silver was added, when an abundant curdy white precipitate of chloride of silver

took place. When the operation was complete, I filtered the solution, collected the washed chloride of silver, and dried and fused it in a small porcelain capsule. It weighed = 3 grs. = chloride of silver = 0.74 gr. chlorine, or 0.76 hydro-chloric acid.

The solution was then cleared of nitrate of silver, by means of hydro-chloric acid, and filtered. Then muriate of ammonia being added, the peroxide of iron was precipitated by pure ammonia, and after washing, drying, and ignition, weighed = 48 grains = 33.28 grains metallic iron.

The oxide of nickel was precipitated by means of a solution of pure potash, and when collected, washed, dried, and ignited, weighed 15.8 grains oxide of nickel = 31.6 per cent. = 24.708 per cent. metallic nickel. After the separation of the metallic oxides, the solution was treated by means of a solution of acetate of barytes, and a white precipitate of sulphate of barytes was formed, which weighed, after washing and drying, = 27 grains = 2 grains sulphur.

The presence of chrome and of manganese having been indicated, I took a separate portion of the meteorite, weighing 10 grains, dissolved it in hydro-chloric acid, adding sufficient tartaric acid to retain the oxides in solution, neutralized the acid by ammonia, and precipitated the iron and nickel, by means of a current of hydro-sulphuric acid gas; after filtration, I evaporated the solution to dryness and burned off the tartaric acid in a small platina capsule under the muffle, when a small quantity of chromic acid was obtained, which was recognized by its characters before the blowpipe; its amount was estimated at 3 per cent. The manganese is also estimated.

From the above analysis, it will appear that specimen 1st of the meteoric iron, having a sp. gr. of 5.750, contains in 25 grains,

| | | | |
|---|---|---|---|
| Metallic iron, | - - | 16.296 = 65.184 per cent. | |
| " nickel, | - - | 6.927 = 27.708 | |

And in specimen 2d, having a sp. gr. of 6.500 in 50 grains, we have

| | | or in 100 grains. |
|---|---|---|
| Metallic iron, - - - | 33.280 | 66.560 |
| " nickel, - - - | 12.354 | 24.708 |
| " chrome and manganese, | 1.625 | 3.240 |
| " sulphur, - - | 2.000 | 4.000 |
| " chlorine, - - | .740 | 1.810 |
| | 49.999 | 99.988 |

It will be remarked, that this meteorite contains an unusual proportion of nickel, and that the occurrence of chlorine, in matter of celestial origin, is here noticed for the first time. I beg leave, therefore, to invite chemists to a careful review of meteorites, since the occurrence of chlorine may have been overlooked in former analyses.

Its occurrence in meteoric matters, is a fact of great importance, in accounting for their chemical phenomena, while passing through our atmosphere.

It must also be remembered, that chloride of iron is readily volatilized at a high temperature, *and that it is abundantly exhaled from the craters of volcanoes*, in various parts of our planet.

Nickel, however, has not to my knowledge been discovered amid volcanic sublimations, but it may be worth while to call the attention of chemists to the subject, that it may be sought for in volcanic craters.

I am however far from believing that we shall be able to prove that all meteorites originate from volcanic sublimations, for there are very evident reasons for believing that our planet, statedly in its course, passes amid numerous detached masses of matter or asteroids, which regularly meet the earth in its orbit on the 13th of November; at least such are the views of Prof. Olmsted, of Arago, and Gay Lussac, whose opinions appear to be supported by the facts which they have collected.

Allowing that meteoric matters are projected from cometary masses, which statedly cross the earth's orbit, coming within the limits of its attraction, and are subjected to the oxidizing influence of the atmosphere, so as to take fire and fall in burning masses upon the surface of the earth, we can more readily account for the phenomena exhibited in their splendid coruscations, when we know that the meteors contain ingredients possessing remarkable decomposing powers, if brought into contact with water or aqueous vapour; and such are the effects of the chlorides of iron and nickel.

In several instances on record, we find the meteor first discovering itself, bursting into fire, from the midst of a dark cloud, and throwing off brilliant coruscations of light, and ejecting ignited masses which fall to the earth; while the globe of fire, from which they were thrown off, traverses the heavens, and gradually becomes extinct. May not therefore the moisture of the atmosphere have first kindled the meteor in its passage through the humid clouds? I do not know whether they are generally too distant from the earth to come in contact with clouds, but from the rapidity of these apparent meteors they cannot be very distant, at the moment of their

conflagration. Should chlorine prove to be a common or constant ingredient, I suppose, that we should have a ready solution of the phenomena involved in the problem.

With respect to the specimen, which forms the subject of the present communication, if we consider its chemical composition, we are forced to regard it of celestial origin; for we have no similar natural alloy in this world, and it contains elements, which are generally found in meteoric matters, besides the new ingredient which I have discovered as one of its components. It is clearly impossible that this mass should have been fictitious; for in all manufactured iron, we can readily detect carbon, which does not exist in our specimen, and the situation in which it was found, is presumptive evidence that it was not manufactured, and the rocks around, not belonging to the class bearing metallic ores, it is impossible for it to have been derived from them, and it could not have been derived from the distant rocks by diluvial transportation, for no such ores exist in any of our mines.

Had it been an ore of iron, reduced by a blast of lightning, we should not have found it alloyed with nickel.

We are therefore led to conclude, that our specimen is of celestial origin, and that it is a fragment of one of those asteroids of cometary matter, which wandering in space occasionally cross our orbit, and being attracted by the earth, so that they rush through our atmosphere, bursting into fire and descending, take up their abode on this sublunary sphere.

*Boston,*
*May 29, 1838.*

---

## LXXIX. *On the Atmospheric Origin of the Aurora and its Connexion with the Crystallization of Snow.* By B. F. Joslin, M.D., *of the City of New York, and late Professor of Natural Philosophy, &c. in Union College, Schenectady.**

New York, 122, Bleecker Street, Aug. 17th, 1838.

*To Prof. Silliman.*

Dear Sir,

There appears to be increasing evidence of an intimate connexion between the aurora and atmospheric vapor, a connexion which has not been wholly overlooked by recent observers. Recent epochs may have been more favourable to its exhibition in the middle latitudes. In the two brief notices of the

* From Silliman's American Journal, for October, 1838.

aurora in the Transactions of the British Association, which met in August, 1837, this is the most prominent feature. In one, Dr. Trail describes the contemporaneous exhibition of stationary cirri and auroral streamers, and in the other, Mr. Herapath attempts to refer the aurora to the precipitation of aqueous vapor. Still earlier, in your own respectable Journal, in a notice of my theory, published in 1836, there was an implied acknowledgment of the existence of some kind of auroral vapor, and even of its magnetic properties.

In March, 1836, there were published Observations on fifty-six auroras, seen by me at Schenectady, N. Y., within the five preceding years, and some new views as to the connexion between this meteor and clouds, rain and snow.*

The author desires to avail himself of the wider circulation of the Journal of Science to communicate to the public some of the principal results, to add others in confirmation of the same views, and to correct a misapprehension which may prevail in relation to the elevation which he assigns to this meteor.† The small elevation which he is supposed to have assigned it is the only objection which he has seen made to his views.

Below is a passage from the paper above referred to, and relates to its different classes of facts, propositions and speculations. It may show that the author has not confounded their different degrees of evidence.

The present article, not having been commenced with reference to any comprehensive theory, presents some miscellaneous facts, which are thrown into the common stock for the use of others. Even among the relevant facts, there are, undoubtedly, interesting relations yet to be traced. 2d. The article contains some generalizations, whose results, whilst they may suggest to others a more correct theory, cannot be thereby invalidated. 3d. There are inferences of another class which may be modified, but probably not overthrown by the progress of discovery. For example, that the aurora is an electrical phenomenon; that it is intimately connected with the elements of clouds; and with these elements only when they are generated in air intensely cold as well as nearly saturated; and that cirrus clouds of a certain class are

* Vide Appendix No. 2, to the Report of the Regents of the University of the State of New York, and the same in a pamphlet of sixty-nine pages, entitled "Meteorological Observations and Essays."

† For the sake of convenience, the term meteor will be used in its more comprehensive sense.

intimately connected with auroral action, and that both these phenomena, and also coronæ, do, for some reason or other, require a cold adequate to the crystallization of aqueous vapor, are propositions which will not lose all their interest nor any of their truth, even if the discovery should be made that the elements of clouds are essentially globular or vesicular, and that the vapor is not yet crystallized at the time of the phenomenon. It may be necessary to remark, that we have not intimated that all snow is not crystallized. On this subject crude notions have prevailed. 4th. As to the views which belong to a more hypothetical class, the author will cheerfully renounce them when a more plausible theory shall appear, as they are designed to facilitate, not to limit, investigation. This theory may contain much that is novel, valuable, and true, without being in the highest sense *the* truth.'

The individual facts on which the generalizations are founded, cannot be here repeated. Of the second class, or the generalizations, are the following

'*Propositions,*

*Which may be regarded as approximately and generally true,* in relation to mean results, though not universally, or in relation to each particular instance.

Proposition 1st, in relation to the *relative time of greatest depression of temperature before different meteors.*

The greatest daily depression or decrement of temperature takes place between one and two days previous to the aurora borealis, auroral clouds and halos.*

Proposition 2d. *Relative order of the thermometric and barometric changes before different meteors.*

Previous to the clouds and halos, the temperature changes either earlier than the pressure or nearly at the same time; previous to the aurora, the pressure changes more than one fifth of a day before the temperature.*

Proposition 3d. *Length of time before the storm† when its indications appear in case of different meteors.*

When the snow or rain is preceded by an aurora borealis, or by luminous columns, the thermometer begins to fall and the barometer to rise between three and five days before the storm; and when the storm is preceded by auroral clouds or halos the same indications are presented between three and three and a half days before it.

* These propositions now stand nearly as they were corrected in the list of errata in many Nos.

† I use the term storm from the want of a better one equally brief, to signify the descent of rain, snow, or hail.

Proposition 4th. *Increase of pressure before rain or snow not preceded by these meteors.*

Previous to a thunder shower, or a rain or snow not preceded by an aurora borealis, a halo, or auroral clouds or luminous columns, the increase of atmospheric pressure for several successive days is less general, but when it does occur, it commences either earlier or later than when the storm is preceded by either of those meteors; more generally between five and a half and six days before the shower or storm.

Proposition 5th. *Time from different meteors to snow or rain.*

The snow or rain descends sooner after a halo than after an auroral cirrus cloud, earlier after this than after a vertical lunar column, and earlier after a lunar column than after an aurora borealis.

Proposition 6th. *Theoretical inference in relation to the nature of these meteors.*

As they are all preceded by a depression of atmospheric temperature below the mean, and by an augmentation of pressure greater than that which precedes the fall of snow or rain at times when none of these meteors have recently appeared, there is additional evidence of the similarity of their origin.

Proposition 7th. *Theoretical inference in relation to their altitude.*

We may infer from the last two propositions that a magnetic cirrus cloud is higher than a halo, but lower than a lunar column, and the latter lower than the aurora borealis.

Proposition 8th. *Practical inference with regard to the prognostication of storms.*

The foregoing propositions which relate to pressure and temperature may suggest a rule for predicting storms much earlier than by other methods; inasmuch as these changes, and especially that of the barometer, take place even more generally than those opposite changes which often occur within the twenty-four hours immediately preceding the storm, and which have been observed by others, and generally regarded as among the surest indications.'

The above propositions are deduced from tables here omitted, and are founded upon the observation of forty auroras, twenty-two auroral clouds, seen in the day time, seventeen halos, and four luminous columns. The propositions in relation to the last and more rare phenomenon, the author considered as entitled to less confidence on account of the small number observed. Yet the optical theory which he gave of it in which he attributes it to a mixture of horizontal,

specularly-reflecting, crystalline plates, with masses which are more amorphous and which produce a reflection virtually radiant, he considers as complete and satisfactory, and corroborated by his observations on the crystals which subsequently descended. The author has observed the aurora in connexion with the above and other meteorological phenomena of the same, the preceding and the succeeding days, and endeavoured to trace their respective and relative changes, and as far as the subject admitted, by the statistical and numerical method. This is a fertile field, and comparatively unoccupied.

In the 3d, or class of inferences, he has endeavoured to show a *connexion between the aurora borealis and the crystallization of snow.*

The following is a summary.

'That crystals of snow more minute and simple than those which occasion halos, and usually too minute to produce sensible opacity, are always present in the atmosphere, above the region of ordinary clouds, during the time of this meteor, we are induced to believe from a comparison of the results of the foregoing observations. Several of these results are believed to be new. The following are some of the circumstances which have a bearing upon this question.

1st. Those seasons of the year and those hours of the night when it most frequently occurs, are favorable both to the presence and congelation of aqueous vapor in the atmosphere.

2d. The clearness of the sky, which at such times is usually either general or total.

3d. The usual northly breeze at the earth's surface, and the northeasterly breeze in the high region of the meteor.

4th. The usual depression of the temperature, at those heights at which thermometrical observations are made.

5th. The clouds which usually succeed the meteor immediately or on the same evening, and which often present the appearance of being continuous and identical with the auroral matter.

6th. The snow that in weather sufficiently cold, almost universally follows the meteor, after such an interval as the simple crystals might be expected to require for aggregation in more complicated groups and descent to the earth's surface.

7th. The rain that almost universally succeeds it, after about the same interval, whenever the temperature of the lower atmospheric strata is sufficient to melt falling snow.

8th. The co-existence of halos with regular crystals, the connexion between halos and auroral clouds, and between

auroral clouds and vertical lunar columns, and the analogy between auroral clouds and the aurora borealis.

"9th. The pinnate appearance of composite auroral clouds, which appear (so to speak) like large crystals.'

"From this point, the author, not finding any former theory of the aurora not liable to great objections, has ventured into the regions of speculation, and in relation to the intimate nature of the phenomenon, and under the 4th head, of views of a more hypothetic class, has ventured to inquire whether atmospheric crystallizations may not occasion the development of auroral light, and the crystals be, under some circumstances, magnetic; and in relation to the 9th remark, has inquired, 'May not this expression be used as something more than a figure of speech? What is so likely to produce this structure, so regular, and yet so complicated, as the polarity of component crystals, whether this polarity is or is not magnetic? May not the ponderable material of the colonnade of an aurora borealis consist of similar groups of crystals, formed either from the vapor of water, or from some lighter, less condensable and more magnetizable vapor in the upper regions, which crystallizes at the same time, and under similar meteorological influences with the former?' Has not the crystalline character of the higher clouds, if it exists, been generally overlooked by meteorologists; and when they have represented all clouds as being masses of condensed vapor, and snow as resulting from its subsequent congelation, have they not overlooked the universally crystalline character of snow, forgotten the small height which is necessary for crystallization, and suffered their imaginations to be influenced by their own temperate climes and moderate elevations.

In advancing a step farther in the attempt at an explanation of the intimate nature of the phenomenon, and especially as connected with aqueous crystals, the author has ventured with diffidence upon a topic still more recondite and obscure, but has found some support in analogies drawn from the electrical light seen during the crystallization of water, from the induction of crystals, and the magnetism developed by changes of temperature in many crystalline substances ordinarily unmagnetic. That iron, probably from its magnetic properties, has a peculiar relation to the crystals of hoar frost, he has been led to suspect, from their tendency to assume a position at right angles to the edges of a magnet and of a tinned vessel, at temperatures between zero and — 12°.

In experiments with the solar microscope, I have been struck with the analogy between the polarity of crystals and that of magnets, a polarity evinced by the rotation of the

smaller groups, in their approach to the larger and more complicated ones. The extent of rotation produced in one group by another never exceeded 180°. I have also detected a still more interesting analogy in the influence which a large group exerts upon the formation of smaller ones at a considerable distance. There was a real *induction*. This was evident from the fact that a large nucleus spread more rapidly than a small one, advancing like a wave, overtaking and absorbing those waves which had begun to spread from a smaller nucleus. This induction, or the influence of a crystalline mass, in disposing particles and small crystals which are in its vicinity, but at some visible distance from it, to unite *with each other*, was still more evident from observing on the screen the existence and motions of scattered clusters composing a darkly dotted border or penumbra, skirting the darker image of the general crystalline mass already formed, and regularly advancing before it across the screen. Perhaps we should hardly be justified in calling such phenomena magnetic; yet it would be easy to show that these and many other phenomena exhibited by microscopic crystals, are regulated by laws strikingly analogous to those of magnetic induction.'

The above phenomena may be shown with great distinctness in tincture of camphor, sufficiently diluted to make the process slow.

If the electricity of crystallizing water is ever connected with magnetism, it must be during the perfect crystallization in the elevated regions of auroral acti
stances are favourable to the perfectio
and the products. The rarity of the v
able to a regular aggregation of the m
is intense. During crystallization, t
crystal might rise to 32°, by the evolut
soon afterwards sinking perhaps 100°
perature of the vapor. For such imme
changes, a less elevation in the air is r
latitudes; and there, it appears from
aurora itself is less elevated. It is u
numerous authorities which exist, to
lowness of the aurora in high latitude
served, that in Faroe and the Shetlan
seen not more than forty or fifty feet above the sea, and learned, that in both countries it is frequently heard. One person had perceived in it, when red, an electrical smell.*

In our latitude, the aurora is usually at great heights. On this subject the author's views seem to have been misappre-

* Edin. Philos. Jour. vii, 162.

hended. Some of the intimate connexions which he has proved to exist, as well as others which he has believed to exist, between the aurora and a certain class of clouds seen in the day time, do not imply an usual identity of location. He had stated, that the aurora is usually higher than clouds, even than cirrous clouds, which are often many miles above other clouds, and many miles above the highest mountains. It by no means follows, that its origin is above crystals of the invisible kind. That the latter may be forming and descending for many hours, and in some instances a day, before they attain such a number, magnitude, and complexity, as to form visible haze, is evident from the phenomena of halos and vertical solar and lunar columns in a clear sky. But these crystals, in their nascent state, must have had a still earlier and higher existence. Should it then be thought surprising, that minute crystals, in a region far above halos, should require a day longer for their aggregation and descent?

It is not my present purpose to discuss at length the question as to the intimate nature of the aurora; but I am of opinion that in some region, usually high, a crystallization takes place on the evening of an aurora, and that the latter originates in the atmosphere. In the publication above referred to, I have ventured to speak of such a thing as "atmospheric magnetism," and to regard it as the direct cause of the needle's disturbance, and as located in a kind of auroral vapor; although it was the prevalent opinion of philosophers, that the aurora, so far as it was magnetic, was connected with changes in tellurian magnetism alone, that is, the magnetism of the solid earth. The variations of the needle were thought to afford evidence of variations in the latter; and this view was thought to be corroborated by some simultaneous disturbances of the needle in distant parts of the globe. Numerous facts might be cited, in corroboration of the atmospheric location. Let one at present suffice. During the brilliant and extensive red aurora of Jan. 25, 1837, I observed at Schenectady, N. Y., a variation of the needle of 1½° in eighteen minutes, of 2¼° in two hours, and 2½° during the night. At New Haven, the variations were, at one hour, still more rapid, that is, 45′ in two minutes; but the whole extent observed was only 1°. About thirty miles north of New Haven, no change whatever could be detected; whilst at Annapolis, the needle varied to the astonishing amount of 10° during the night.* Are not these facts wholly irreconcilable with the idea, that the needle was

* See this Journal, Vol. xxxii, p. 180.

disturbed by a general change in the magnetism of the earth? According to Capt. Back, auroral beams sometimes seem to attract each other. Does not this seem like atmospheric magnetism?

There appears to be no reason to believe that the aurora is at an invariable elevation. Calculations founded on observed altitudes, have given results varying from a few miles to several hundred. This discrepancy may be explained, partly by an actual difference of height, and partly by mistakes as to the identity of arches when several have been presented to different observers. In the latter case, a mistake will usually lead to an exaggeration, rather than to an underrating of the elevation. Suppose two observers, near the same meridian, but in different latitudes, to take the altitudes of two arches situated north of their respective observers, and at so small an elevation, that the southern arch is below the horizon of the northern observer, and the northern arch below the horizon of the southern observer. Only one being seen by each, they are liable to be presumed identical; and the great altitude of the northern as compared with the southern arch, would lead the mathematician to refer the imaginary arch—considered as one—to an elevation greater than the actual elevation of either of the real arches. There is evidence that the above case is more than a supposable one, and that similar mistakes have actually occurred. The opposite error, an exaggeration of the parallax, would, from the nature of the case, more rarely occur. I have stated the first in a plain way, that those who are little conversant with the subject may not be deterred from examining the physical evidence of a theory of the aurora, by a caveat supposed to have been entered by the exact sciences. There are facts quite as conclusive as a great parallax: such as the numerous instances where individuals at moderate distances cannot recognize the same phases, and some of them not even the existence of the aurora seen by the others. In such cases, it may fail to be measured, simply because it is too low.

The views which I have taken of the aurora, whilst they do not require us to discredit those numerous proofs, both physical and mathematical, of its occasional situation in the inferior atmospheric strata, at the same time, allow, or even require us to refer it in most instances to elevations above (and in the lower latitudes far above) the regions of the highest proper clouds, and many times as high as ordinary clouds. Physical considerations have induced me to refer its origin to the earth's atmosphere. The height of this is well

known never to have been determined, so far as respects those rarer portions which reflect no sensible light.

Those who reflect, that there is a depression of about 1° for every 300 feet of elevation, will find little difficulty in admitting the existence of crystals of snow above us in summer. The following facts have a bearing on this, as well as on the connexion between the aurora, snow, and magnetism. On the 16th of August, 1836, I observed, at Schenectady, an aurora, at 10 P. M., chiefly obscured by clouds, and a faint aurora with three or four short streamers extending to the height of $\gamma$ Ursæ Majoris, at 2h. 10m. next morning. The sky was clear, and remarkably so during the forenoon. At 7 A. M., the magnetic intensity was high and remarkably variable; the time required for 100 oscillations of a suspended needle being 270 seconds at 7 o'clock, and 280 ten minutes later. Rain commenced at 9 P. M. of the 18th, about two days after the first appearance. Quantity during the night, .32 inch. On this day the 18th, an aeronaut, Mr. Lauriat, who ascended from New York over Long Island, encountered what was called by the papers "a pretty severe snow storm in the upper regions; and when he touched terra firma, his clothes were frozen stiff.* The crystals may have been minute. The following is from another paper, and may perhaps refer to the same ascension. "In Mr. Lauriat's last ascension from New York, he ascended about five miles, and proceeded over a hundred miles. He passed through clouds of sleet, which covered his balloon with icy particles. But what was more interesting, he discovered that when he was at the greatest altitude, the needle of a compass which he had with him did not have the least tendency to exhibit polar attraction, but wavered about at all points of the compass."† May we not conclude, that the atmospheric magnets at the height of five miles acted more powerfully than the earth? Even at the surface, I have inferred, from many hundred observations, that the magnetic intensity is more affected by the formation of the higher clouds, and other obvious crystallizations, than by any other periodical diurnal changes.

The following facts have also an interesting bearing on the theory. At Fort Enterprise, where Lieut. Hood found the aurora in one instance to be only 2¼ miles high, he was, in two instances, surprised to see a discharge of snow, in small flakes, from a clear sky, at times when the aurora was active

* New York Commer. Adver. of August 19, 1836.

† Middlebury Free Press, of Nov. 29, quoting the Boston Herald —date not given.

near the zenith.* These facts, with existing theories, were then extremely puzzling; but they are in exact accordance with the above theory. The short interval before the snow, and the diminutive flakes, are what might be expected in case of an extremely low aurora. Lieut. Hood's measurements and observations will not be disputed.

As early as 1820, (April 3,) my interest in the subject of the connexion between the aurora and apparent clouds, was excited by a beautiful white arch, like a roll of wool, which on that evening was seen to detach itself from the summit of an aurora of the ordinary character, and in the rapidity of its motion toward the zenith, in the distinctness of its texture as it approached it, in the resemblance of this texture to that of a fleecy cloud, and in other circumstances, seems to have been unlike any arch in an elevated region.

Subsequently, an interesting class of objects of a more decidedly nepheological character, but still intermediate between the aurora borealis and ordinary clouds, has presented itself in polarized, linear cirri, or magnetic or auroral clouds. The linear cirri, when of great extent, and in other respects of a regular character, have generally been either in or near the magnetic meridian, or nearly at right angles to it. In hundreds of instances, these positions are within a degree or two of them. These can hardly have been accidental coincidences, and they have had no constant relation with wind. In epochs marked by auroras, these have been more marked. They are occasionally composite, consisting of an arch with rays, like streamers. Whence the polarity of these clouds? They open an interesting field, and establish a curious analogy between the aurora and the phenomena of the lower regions. Although the N. and S. delicate lines correspond with auroral streamers, in their coincidence with the meridian, yet the author has not confounded them, but has shown that the former differ from the latter in the absence of the dip.

But the analogy is not restricted to position. It was soon detected in the concomitant phenomena. I have shown, by tabular views, that the thermometer usually begins to fall, and the barometer to rise, several days before each, and rain or snow to descend within one, two, or three days after them. In the cases subsequently presented, in which the number of hours between the aurora and storm has been carefully noted, I have usually found that the time has been about thirty-six hours, and that there is a curious exception in the case of two auroras on two or three consecutive nights, in which case, the

* See appendix to Franklin's Journey to the Polar Seas.

rain or snow is least likely to descend, or is deferred till nearly the usual time after the last. The same is true of the polarized clouds, and of halos; in both of which, vapor, which had unquestionably been precipitated, is redissolved, or otherwise disposed of, during the time and under the influence of the circumstances preparatory to or attendant on the second exhibition.

This interference of one aurora with the results of its predecessor, opens a curious field of investigation, discloses a new analogy between this and meteors of a confessedly aqueous origin, and refers to a general law the observed exceptions to the descent of precipitated vapor which so generally takes place after an aurora. In almost every instance in which this has been deferred, there have been traces of auroral action on the succeeding night, though sometimes masked by the moon. The following rule has had few exceptions, viz. If the evening of the day after an aurora is totally clear, no storm follows on the second day; and conversely, if no storm is to follow, this evening is totally or nearly clear. This general clearness is itself one of the usual attendants of auroral action; and I have for many years observed, that the morning following an aurora is, in this respect, remarkable, as compared with other mornings. In this fact, and in the unusual clearness of the night of the meteor—with the exception of some peculiar, transient clouds—we have proof of the influence of an aurora, or the circumstances which precede and attend it, in effecting the resolution or disappearance of visible vapor, or precipitations.

This enables us to explain or generalize the fact of the non-appearance of the storm, of which the first of two consecutive auroras would have been the precursor. As tending to elucidate this new and interesting field of enquiry, I will state the results of observations on thirty-two auroras, observed at Schenectady, N. Y., between Oct. 5, 1830, and Nov. 3, 1833,—the tables being prepared for these alone, although the results of subsequent observations are, I am persuaded, not less striking. My observations are made at 9 A. M. and 9 P. M. The proportions of sky clear at the times of observation, are set down in tenths. About one day before an aurora, the sky usually begins to increase in clearness. In the following results, reference was had only to clearness as compared with the corresponding hour of the preceding day, and only to mean results. During the 24 hours preceding the morning of the day on the evening of which the aurora occurred, the sum of the increments of clearness was to that of the decrements as

two to one.* During the 24 hours immediately preceding the aurora, the increments are to the decrements as six to one. Similar results would be obtained by taking the number of instances in which the clearness increased or diminished in case of different auroras, instead of the amount of tenths, as above. Within the two days preceding an aurora, and on some part of the night of it, we observe all the circumstances preparatory to and connected with crystallizations in the high regions, developing themselves; such as increasing atmospheric pressure, increase of cold, and the disappearance of clouds. On the other hand, during the day or two succeeding it, are developed all those circumstances which attend a more advanced stage and lower descent of the products, whether crystalline or melted; such as a diminution of atmospheric pressure and clearness, and an elevation of the temperature and dew point. The latter changes, occupying less time, are more rapid than the former, and hence appear more striking. For example, during the 24 hours succeeding an aurora, the decrements of clearness are to the increments as 37 to 1. But this high ratio requires in reality to be further increased, in conformity with the principles above established. For, the principal increase of clearness which occurred, was in a single instance, and that on the occasion of two consecutive auroras, the latter tending to prolong and increase the clearness. This instance being omitted, as it should be, the decrements of clearness during the 24 hours succeeding the aurora are to the increments as 112 to 1, the increment having been in one instance one tenth, and the whole decrement in thirty instances 112 tenths. On none of the eight instances in which there were auroras on two consecutive nights, had the cloudiness increased on the evening of the second, as compared with that of the first. The mean decrement of clearness for the remaining 24 instances was 46. Hence, to give a popular statement, approximately true, the evening of an aurora is, on an average, twice as clear as the succeeding evening, unless another aurora occurs on the latter, in which case, the sky continues equally clear. As the forenoon succeeding an aurora is in general unusually clear, this great decrement of clearness usually takes place in a few hours, whilst the increments had required several days.

* The sum of the tenths which respectively express the amount by which the sky became clearer on the respective days immediately preceding the different auroras, is called, in expressing the mean results, the sum of the increments during the 24 hours immediately preceding the aurora. A similar expression is used for other epochs and for the decrements.

The following table, (abstracted from those on which the nine propositions are founded,) shows the mean temperatures at 9 P. M. of the days of the different meteors, and on the evenings one and two days previous; also the mean number of days previous, when the changes of pressure and temperature commenced.*

The number of vertical beams is so small, as to forbid confidence in mean results as to elapsed time. In the case of the other meteors we see a pretty near correspondence as to the times when the thermometric and barometric changes commenced before them all; and find, in the relative temperatures required for them, a corroboration of the conclusion drawn from the time of the succeeding storm in relation to their relative heights in the air.

The absolute temperature is, for the seasons of their occurrence, low for all, and of itself affords evidence of the existence of crystals. From semi-monthly observations for five years, on two springs at Schenectady, I have inferred, that the mean temperature of the earth there is 48.5°, and this accords nearly with the mean temperature of the air in that vicinity for the last ten years. Should we make allowance for the daily mean, and for the mean seasons of the year in which the aurora occurs, we should have a still more just and striking view of the cold usually required for its production. The barometer rises and the thermometer falls before an aurora, and the mean length of time is about two days; and, consequently, these changes commence about four days before the storm, or about three and a half days when there are not two auroras in succession.

This affords one of the earliest and surest prognostics of the storm, and is more to be relied on than even the subsequent depression of the barometer, which, in modern times

* Certain errors which, through the inadvertence of an assistant, had crept into the tables, are here corrected.

[illegible] It would be curious, (though [illegible]) and I have not
nal,) if this early ascent of the barometer were
o in the long since banished rule of Pascal.
atriarch of this branch of science may, as is
fallen into a grave error in regard to this, yet
erived a certain modification of his rule, that
rises before a storm; and perhaps he may be
e error and prove to be the original discoverer.

That the changes of pressure and temperature commence before the aurora, accords with the above theory. They are to be regarded as among the causes rather than the effects of the aurora. Yet that they continue a little beyond the time of it, I have long since observed, and expressed it by the rule, that the barometer is usually rising, and the thermometer falling, on the evening of an aurora.

Within a few years, an interesting confirmation of the above theory, so far, at least, as to the fact of a connexion between atmospheric vapor and magnetism, has been presented in
es, at different times, in a peculiar deep blue,
, cloud, resting on the horizon in the north, in
its centre of gravity being exactly, or almost
magnetic meridian. Whenever the cloud was
ue color, its direction was taken by the compass;
any bias from preconceived theory, a point
he centre of gravity was selected, previous to
e needle. The variation from the meridian rarely exceeded a fraction of a degree; the correspondence in direction being more exact than that of the position of most polarized clouds. Had the writer been influenced by love of theory, he might have wished the latter and more explicable phenomenon to be the more regular of the two. He would invite the attention of more northern observers to this somewhat mysterious phenomenon, should the return of auroral epochs reproduce it. To those less favorably situated, he may appear to have drawn upon his imagination. Did time and space permit, he might give more particulars. He hopes occasionally to resume this and kindred subjects, so far as his present residence in a latitude less favored by auroral exhibitions, and his more exclusive devotion to professional duties, will allow.

## LXXXI. *On the Liquefaction and Solidification of Carbonic Acid.* By J. K. Mitchell, M.D.*

In the year 1823 public
the subject of the liquefactio
permanent gases, by Mr., nov
the aërial fluids, carbonic acic
force of 36 atmospheres at 3
state. His ingenious and h
ducted in glass tubes; and
of newly generated gas for the necessary pressure.

Mr. Brunel,‡ in a subsequent endeavour to apply compressed gases to mechanical purposes, produced a pint and a half of liquid carbonic acid, which, even at high temperatures, he confined in a series of small brass tubes not above the $\frac{1}{16}$ of an inch in the thickness of their walls.

This interesting subject was not again publicly agitated, until the appearance in December, 1835, of a report on the liquefaction of carbonic acid on a comparatively large scale. In the last number for that year of the *Annales de Chimie et de Physique*, M. Thillorier described the properties of liquid carbonic acid in detail. Acc
demands for its existence as such
stated by Sir M. Faraday, of 36 a
gravity is at the same temperatur
and at 86° — 0.600. It is therefor
times as much as its own or any oth
32° to 86°. From — 4° to 32° its e
equal to that of the gases.

M. Thillorier found also that the
by heat so as to amount at 86° to 7
4 to 26 atmospheres. The density
over the liquid at 86°, is stated at
that which is compressed by the
Its pressure is therefore at 86° not
of that which its density would indicate.

When liquid, the carbonic acid is, on the sa
immiscible with water and the fat oils, but is i
with ether, alcohol, naphtha, oil of turpentine, a
sulphur. Although potassium decomposes it, le
per, and the other easily oxidized metals, do not

* From the Journal of the Franklin Institute, for November, 1838.

† Philos. Trans. Lond.

‡ Quart. Jour. vol. XLI.

§ Among the most remarkable of the phenomena observed by Thillorier was the intense cold produced by the sudden liberation of the liquid and its conversion into gas. A jet of it depressed the

The theometric temperature, observed in the jet by Thillorier, appears to be erroneously stated; for, as the *solid* is, at its formation, not below $-90°$, and as the act of solidification of any vapour or liquid keeps the temperature, for the time, at the highest point compatible with the existence of the particular solid under observation, it follows that the jet of carbonic acid cannot fall below its freezing point. Immediately after its production, the carbonic snow begins to grow colder, and may be made to reach $-109°$ in the air, $-136°$ under an exhausted receiver. When moistened with ether, it can be depressed to $-146°$. Professor Hare's ether acts much more effectually than sulphuric ether.

At the immediately subsequent sitting of the Academy of Sciences, Thillorier announced the important fact that he had solidified carbonic acid. This he effected by suffering the liquid to escape into a bottle, or box, where by the sudden gasefaction of a part, the remainder was frozen by the extreme cold thus produced. The solid is white, light, evaporable and excessively cold. Because, surrounded by an atmosphere of gas which is constantly escaping from it, a fragment of it touched lightly by the finger glides rapidly over a plane surface.

Its evaporation is so complete as to leave no other trace of moisture than that which is caused by the coldness and consequent atmospheric humectation.

The force of its gasefaction is alleged to be equal to, but not so sudden as, that of gunpowder.

The temperature at which the solidification took place was presumed to be about $-148°$ F.; although the experiments before the committee of the Academy shewed $-124°$.

Such is, in substance, the account by M. Thillorier of his novel and curious discovery, reported in the *Annales de Chimie*. No description of the method of procedure, or of the apparatus used, is annexed; and we are left to conjecture, and to the imperfect description of travellers, for any farther knowledge of either.

Having repeated the experiments of Thillorier, I deem it not useless to subjoin a draught of the instrument with which, aided by the suggestions of an intelligent pupil in France, and the assistance of friends here, I was enabled successfully to repeat most of the experiments of Thillorier and to verify some, and correct other, of his results.

The apparatus, fig. 3, Plate XVI, consists of a generator of cast iron, A, supported by a wooden stand, B, a receiver,

thermometer to $-130$ F., and when sulphuric ether had been previously mixed with the liquefied gas, the refrigerating effects were more marked both on mercury and the sensations.

F, also of cast iron, connected to the generator by a brass tube, and fastened firmly to it by the stirrup screw, K; H, I, J, are stop-cocks, G the nozle of a pipe, L a glass level-gauge, S, M, R, a pressure-gauge.

The generator is 20 inches long and 6 inches in diameter exteriorly. Its cavity is 16 inches deep and 3 inches, nearly, in diameter, so that it will hold about 4 pints. The walls are, of course, about $1\frac{1}{2}$ inches in thickness. At the top an aperture of two inches in diameter is closed by a strong wrought-iron screw, the shoulder of which is let in about a quarter of an inch. The collar is of block tin turned to the size of the shoulder of the screw. There is a hole in the head of the screw, E, for the reception of a long, strong, iron bar.

The copper cup, N, $1\frac{3}{4}$ inches wide, and 9 inches long, holds about 12 fluid ounces. There is a little handle at the top, and a copper wire at the bottom which make the whole length a little less than that of the cavity of the generator. This cup is used to introduce the sulphuric acid.

The brass tube between the generator and receiver is divided into two parts of equal length, which admit of being united by means of a conical juncture, kept tight by the stirrup and screw, K, K. Each of these portions of the tube may be closed or opened at pleasure by a stop-cock. One is placed at I, another at J; so that when the receiver is being separated from the generator, the contents of both may be retained. The stop-cocks in common use are inadequate to resist the pressure, and, therefore, a screw stop-cock is indispensable. It is made to close a small aperture by means of a conical point, and having a double cone, it closes an outlet also when the cock is completely open, so as to prevent the escape of gas by the sides of the screw.

The receiver, F, is of the capacity of about a pint. The pipe, G, G, turned at a right angle at G, descends so as almost to touch the bottom of the cavity in F. The stop-cock H, G, is similar to I and J. L is a glass tube connected at each end to a socket of brass, which communicates with the interior of F. It is the gauge for observing the level of the liquid in F.

The gauge for measuring the pressure is peculiar. Into a wrought iron box, S, are inserted, by screws, two sockets, T and U. The former descends almost to the bottom of the box, which is nearly filled with mercury. Through the axis of the screw, X, a small tube passes into the cavity of S, and is continued to the top of it, so as to rise above the mercury. Two strong barometer tubes, R and M, are cemented,* into

* The cement used was made of shell lac 3 or 4 parts, white or crude turpentine 1 part, melted at as low a temperature as possible

U and W, and hermetically sealed at the upper ends. These tubes are carefully graduated. In one of them, U, a short cylinder of mercury is made to stand at Y at the commencement of the experiment. The other, socket and all, is full of air, as no mercury is introduced into it. A very fine screw at W, enables the operator to regulate the quantity of air in T.

The tin cup, O, used to collect the solid acid, is covered by a lid, Z, perforated by a pipe, P, whose top is full of small holes. The handle, Q, is hollow, so as to fit the end of the pipe of the receiver at G. To secure the hand of the operator from the cold produced by the experiment, the handle is carefully wrapped up in some kind of cloth.

The apparatus is prepared for use by removing the screw, E, and placing $1\frac{3}{4}$ lbs. of bicarbonate of soda in the generator, A, to which 24 fluid ounces of water are to be added. After making these into a thin paste by stirring, nine fluid ounces of common sulphuric acid are to be poured into the copper cup, N, and that is to be let down by a crook of wire into the generator. After the screw, E, has been firmly applied, and the stop-cock, J, closed, the contents of the generator are to be brought into admixture by moving it round to a horizontal position on the swivel, D, which is supported by the wooden frame, B, B. There is a check bar at C. This motion is to be repeated several times. In about 10 minutes the whole of the carbonic acid is liberated, and exists in A, chiefly in a liquid state.

The next step in the process is to attach by means of the stirrup and screw, K, K, the receiver, F, *previously cooled by ice.* The keys, I and J, may then be opened slowly, and instantly the liquid carbonic acid is perceptible in the gauge, L. At the end of ten minutes the communication with the generator may be cut off—when about eight fluid ounces of liquid acid at 32° F. will be found in the receiver.

By letting this liquid into the box, O, through the pipe, G, a large part of it is instantly expanded into gas, which escapes through the tube, P. The coldness consequent on the enormous expansion freezes another part of the liquid, which falls to the bottom of O. About one drachm of solid matter is thus formed for each ounce of liquid.

The porosity and volatile character of the solid renders its specific gravity of difficult ascertainment. When recently

so as not to make bubbles in the mixture. This cement is very strong, but liable, without great care in the regulation of the heat, to have capillary tubes in it from the vaporization of the turpentine. This defect may be completely corrected by cutting away, when cold, the external mass of cement, and putting on a little common cap cement which melts at a much lower temperature and closes the tubes.

formed it is about the weight of carbonate of magnesia, and when strongly compressed by the fingers, its density is nearly doubled. Solid carbonic acid is of a perfect whiteness, and of a soft and spongy texture, very like slightly moistened and aggregated snow. It evaporates rapidly, becoming thereby colder and colder, but the coldness produced seems to steadily lessen the evaporation, so that the mass may be kept for some time. A quantity weighing 846 grains lost from 3 to 4 grains per minute at first, but did not entirely disappear for 3 hours and a half. The natural temperature was 76° — 79°. The solid is most easily kept when compressed and rolled up in cotton or wool. Its temperature when newly formed is not exactly ascertainable because it is immediately lowered by evaporation. Thillorier seems to have entertained the opinion that the greatest degree of cold was created at the time of the formation of the solid. In my experiments a constant decrease of temperature was observed; which was accelerated by a current of air, or any other means of augmenting evaporation. At its formation, the carbonic snow depresses the thermometer to about —85°. If it be confined in wool or raw cotton, its cooling influence is retarded; if it be exposed to the air, especially when in motion, the thermometer descends much more rapidly; and under the receiver of an air pump, the effect is at its maximum. The greatest cold produced by the solid carbonic acid in the air was —109°, and in an exhausted receiver —136°, the natural temperature being at +86°.

The admixture of sulphuric ether so as to produce the appearance of wet snow, increased the coldness, for the temperature then fell, under exhaustion, to —146°,* a degree of cold which we were not able to exceed by means of any variation of the experiment. That result is most easily obtained by putting about two fluid drachms of ether into the iron receiver before charging it. A compound liquid may be thus formed which yields a snow in less quantity, but of a more facile refrigeration. Alcohol may replace ether in either mode, but with less decided effect. In the air the alcoholic mixture fell to —106° and remained stationary. By blowing the breath on it, it fell to —110°. Left to itself it rose slowly to —106°; but on being placed under an exhausted receiver it fell to —134°.

Every attempt to wet the carbonic solid with water, failed, so that no estimate of its relative effects could be made.

* As —146 + 32 = 178, the cold is nearly as far below the ice-point as 212 — 32 = 180 is above it.

The experiments resulting from the great coldness of the new solid, were very striking. Mercury placed in a cavity in it, and covered up with the same substance, was frozen in a few seconds. But the solidification of the mercury was almost instantly produced by pouring it into a paste made by the addition of a little ether. Frozen mercury is like lead, soft and easily cut. It is ductile, malleable, and insonorous. Just as it is about to melt, it becomes brittle or "short," and breaks under the point of a knife. These facts may account for the discrepancies of authors on this subject. Frozen mercury sinks readily in liquid mercury.

At about —110° liquid *sulphurous acid* is frozen, and the ice sinks in its own liquid, and at —130° *alcohol* of .798, assumes a viscid and oily appearance, which by increase of cold, is augmented until at —146° *it is like melted wax.* Alcohol of 820 froze readily.

At —146° *sulphuric ether* is not in the slightest degree altered.

When a piece of solid carbonic acid is pressed against a living animal surface, it drives off the circulating fluids and produces a ghastly white spot. If held for 15 seconds it raises a blister, and if the application be continued for 2 minutes a deep white depression with an elevated margin is perceived; the part is killed, and a slough is in time the consequence. I have thus produced both blisters and sloughs, by means nearly as prompt as fire, but much less alarming to my patients.

The specific gravity of liquid carbonic acid may be estimated either by weighing a given measure of it in a tube, and deducting the weight of the tube, and of the superincumbent gas, or by means of very minute bulbs of glass as suggested by Sir M. Faraday. By the latter means I obtained the following results, which are compared with those of Thillorier.

| | | Thillorier. | |
|---|---|---|---|
| Temp. Fahr. | Sp. Gr. | Temp. Fahr. | Sp. Gr. |
| 32° | .93 | 32° | .83 |
| 42°.5 | .8825 | | |
| 51° | .853 | | |
| 74° | .7385 | | |
| 86° | | 86° | .60 |

The specific gravity particularly at 32°, was examined repeatedly, and with different bulbs, and always found to be at, or very near, to .93. The difference never amounted to .005. The sp. gr. as given by Thillorier at 32° is .83. The anomalous expansion of the liquid as indicated by both sets of experiments is truly surprising. By mine 73.85 parts raised from 32° to 74°, or 42°, become 93 parts, and gain

19.15 parts, while the same bulk of the gases acquires in the same range of temperature only 6.46 parts, or the liquid is expanded very nearly three times as much as its own or any other gas. According to Thilorier, 60 parts gain 23 parts by an elevation of 54°, while the same bulk of air would under like circumstances be augmented only by 6.75 parts; or the liquid is nearly four times as expansive as the gases.

As below 32°, or at reduced pressures, the augmentation of temperature is productive of much less expansive influence, we may infer that under the weight of a few atmospheres, as when near to its freezing point, liquid carbonic acid is scarcely more dilatable by heat than water. Between −4° and +32°, its expansion is 0.053 while that of air is 0.069. These facts suggest the enquiry how far water at *very high temperature and pressure* may be obedient to the same expansive influence, and thus by suddenly filling the whole interior of a boiler, sometimes cause explosions.

The pressure of carbonic acid gas, when placed over its liquid, is given by Thilorier at 32° and 86°, as 36 and 73 atmospheres respectively. By means of the gauge S, M, R, I found the pressure as follows:

| | | |
|---|---|---|
| 32° | 36 | atmospheres. |
| 45° | 45 | do. |
| 66° | 60 | do. |
| 86° | 72 | do. |

The principle of the gauge renders it capable of registering the pressure with great accuracy:—for as one tube, M, begins to mark the pressure from the commencement of the experiment, and the mercury in the other, R, does not reach a visible point until the first has shown a pressure of several atmospheres, the second tube is equivalent in effect to one of several times its length. The first determines the amount of pressure, at which the mercury reaches the initial point on the 2d, and the 2d, subsequently, exhibits the multiplicators of that initial quantity. Thus, if when the mercury is at five atmospheres in M, it is at the unit mark in R, the value of that unit will be five, and the numbers representative of the pressure on R, must be multiplied by five; or R is equal in effect to a tube of five times its length. By these means very short tubes may be used to determine very high pressures. Inequalities in temperature, irregularities in the cement, and other causes, may vary the capacity of the socket T, W, but as M always signifies the unit for R, in each case, no error can arise from these causes. There must, of course, be a correction for the weight of the mercurial column in R, which is to be added to the product. Care must be taken to keep the

temperature of the vessel which holds the liquid below that of the gauge and tubes, otherwise the liquid will be formed by condensation in the latter. This actually happened in the attempt to ascertain the pressure at 86°, when the natural temperature was 75°. Bubbles of gas were seen ascending through a liquid in M, up to its surface at a few inches below the mercurial cylinder. This as far as relates to the tubes may be avoided by prolonging the socket of M, down into the mercury of the cup, so as to include a cylinder of common air between two cylinders of mercury, and prevent any carbonic gas from entering either the socket, or the glass tube. A correction for the weight of this column, must in such case be made.

When a glass tube, hermetically sealed at one end, and cemented into a brass socket and screw at the other, is attached to a charged receiver and cooled by snow or pounded ice, liquid carbonic acid may be collected in it. It is perfectly colourless and transparent, and the specific gravity bulbs, previously introduced, are seen to ascend or descend, as the temperature is altered. When the tube so charged is opened, the liquid becomes violently agitated, escapes rapidly, grows colder and colder, and finally the remainder is converted into a solid, more dense than the snow already described, but nearly white, and very porous. If the tube be exposed to a paste of carbonic snow and ether, the liquid is solidified into a mass which is not porous but which sinks in the liquid as the latter is formed again by the melting of the solid.

The analogy between liquid carbonic acid and water, is thus completed for we have liquid, vapour, snow, and ice, exhibited by both.

By the previous introduction of water, ether, alcohol, metals, oxides, oils, &c. into such tubes, and then filling them with liquid carbonic acid, the resulting phenomena may be easily observed. Water being heavier rests below the new liquid, and does not appear to mingle with it even at the surface of contact; for when the latter is let off, no bubbles appear in the water, and it is frozen at the top into a solid ice.

When alcohol or ether is introduced, the new liquid falls through it in streams, as water would do, but soon renders it milky by mixture. The removal of the pressure causes a violent effervescence, and immediately the clear, colourless ether, or alcohol, is seen alone in the tube; no solid being formed. When alcohol holds shell lac in solution, the acid causes its precipitation in light whitish flocculi, which are immediately re-dissolved when the acid is suffered to fly off. Nothing remains but the brown lac-stained liquid.

Liquid carbonic acid did not appear to act on any of the metals or oxides, but the experiments on this point demand a further examination. Its inaction is probably owing to the want of the force of 'presence,' or of 'disposing affinity.'

When the liquid has been frozen in a tube of glass, the tube may be melted off by the blow pipe, and hermetically sealed. Such a tube will always retain the liquid, or gas, the former, if in sufficient quantity, at all temperatures, if not, the latter alone will be found in it at high temperatures. I have one such tube, which begins to show moisture at 56°, and exhibits a constantly elongating cylinder of liquid, as the coldness is increased. At 32° the cylinder is about half an inch in length.

Carbonic acid, mechanically powerful as it is, is not applicable, perhaps, either to locomotion or projection; but though the reasons for this are most of them obvious, the Franklin Institute has appointed a committee to investigate and report on the subject, that the exact truth may be known, and the waste of time and talent likely otherwise to be experienced, be saved to the country.

---

## LXXXI. MISCELLANEOUS ARTICLES.

### *Description of a Voltaic Battery, in a letter addressed to the editor of the Annals of Electricity. By* T. B. Spencer, Esq.

11, Montpelier Row, Blackheath,
Thursday, Feb. 27, 1839.

Dear Sir,

Being a constant subscriber to your excellent Annals, and having devoted a good deal of time to the subjects treated of therein, I avail myself of your offer, often expressed, of noticing any little improvements in apparatus or means of rendering them easy of construction to those who, like myself, always frame their own instruments. My improvement is on a battery usually assigned to Professor Daniell, in which earthenware tubes are employed; the objection to which is, that they are very easily broken, difficult to procure except in London, and very dear, and which detracts from the merit of an instrument which otherwise far surpasses any other as a sustaining battery. All these objections are removed by substituting brown paper for earthenware, and which works better in every point. I give you a diagram in hope that it may be of use to some of your readers.

I remain,
Yours, &c.
T. B. SPENCER

A, Fig. 4, Plate XVI, a piece of wood with a hole cut in it, grooved round the edge. B, a piece of solid wood with groove. The dotted lines show the paper, of which there are from 3 to 6 turns, secured top and bottom with twine, or thin copper wire. C, amalgamated zinc plate acted on in A B with diluted sulphuric acid. D, copper vessel holding sulphate of copper in solution. E, colander made of sheet copper, bent and dropped down 3 inches into D.

---

### *Electro-Filtration of Water through porous bodies.*

In a letter from R. Were Fox, Esq. dated Falmouth, March 23, 1839, I find that that gentleman has obtained similar electro-filtrations of water to those described at page 493 of the present volume, by the employment of single voltaic pairs only. Whilst speaking of his interesting experiments with copper ore and zinc, with intervening clay partitions, he observes, "In these experiments most of the water in the zinc cell is transferred *through* the clay, to the copper cell, and if the supply of water be kept up in the former, the liquid in the latter will overflow in abundance. This effect is, perhaps, still more remarkable when a portion or vessel of unglazed earthenware is used. I infer from these experiments, that the electro-negative ores in the earth may, in connexion with more electro-positive metalliferous bodies at greater depths, determine the ascent of water and metalline salts towards the former, and the descent of the acids towards the latter. Iron and copper pyrites, the protoxides of iron, &c. &c. are electro-negative with respect to the metals, and it can be proved by experiment that they will act on each other at great distances *through strata*, if *the latter be wet*; so that the geological bearings of their process, and its influence on the circulation of water under the surface, may be very important."

END OF THE THIRD VOLUME.

---

W. Annan, Printer, 12, Gracechurch Street, London.

AN N PL. III.

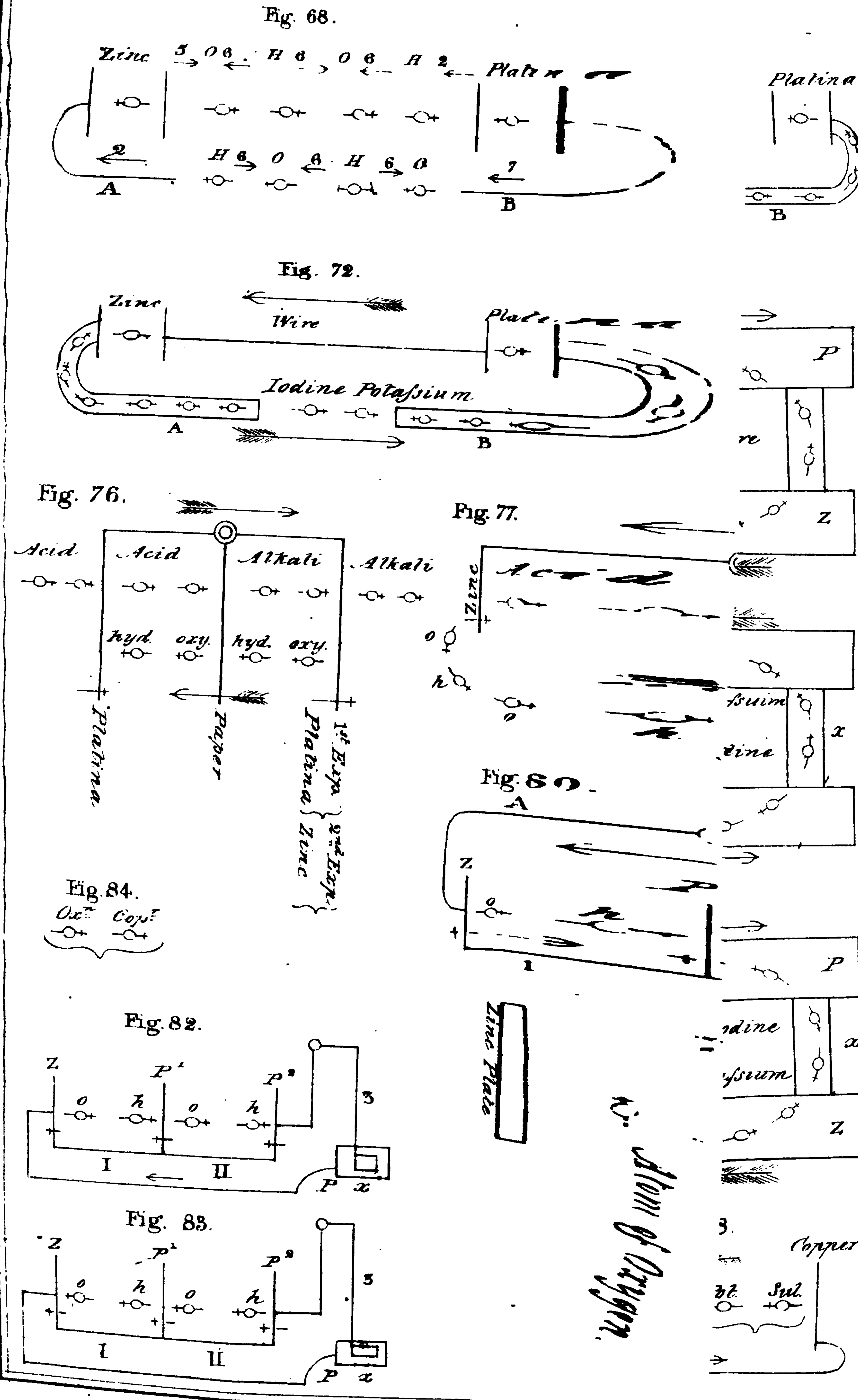
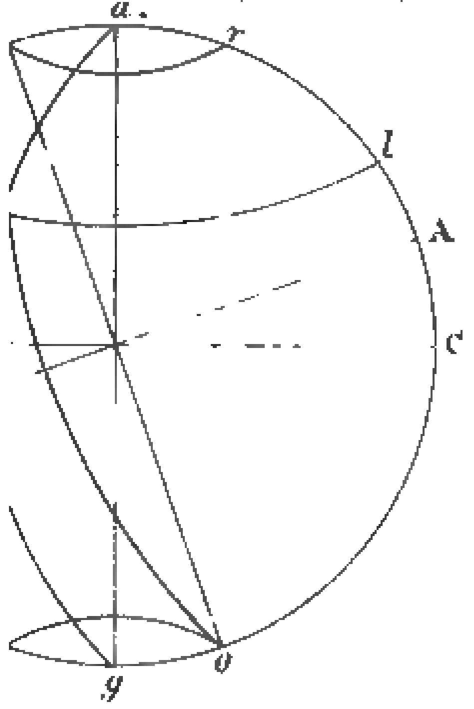


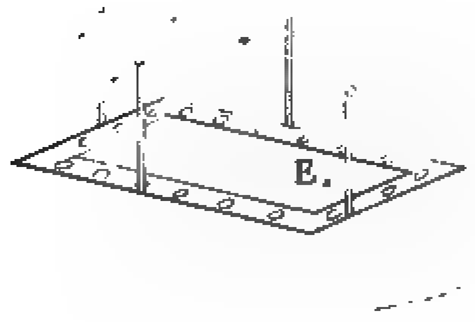
Fig. 68.
Zinc
Platina
A
B
Fig. 72.
Zinc
Wire
Iodine Potassium
A
B
Fig. 76.
Acid
Acid
Alkali
Alkali
hyd.
oxy.
hyd.
oxy.
Platina
Paper
1st Expt. Platina
2nd Expt. Zinc
Fig. 77.
Zinc
Fig. 80.
A
Z
Fig. 84.
Oxn
Copr
Fig. 82.
Z
P1
P2
3
I
II
P
x
Fig. 83.
Z
P1
P2
3
I
II
P
x
Zinc Plate
Atom of Oxygen.
Z
x
P
Copper

ANNALS OF ELECTRICITY &c

PLATE XVI VOL III


part of Fig 4



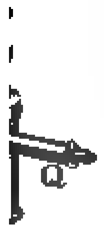
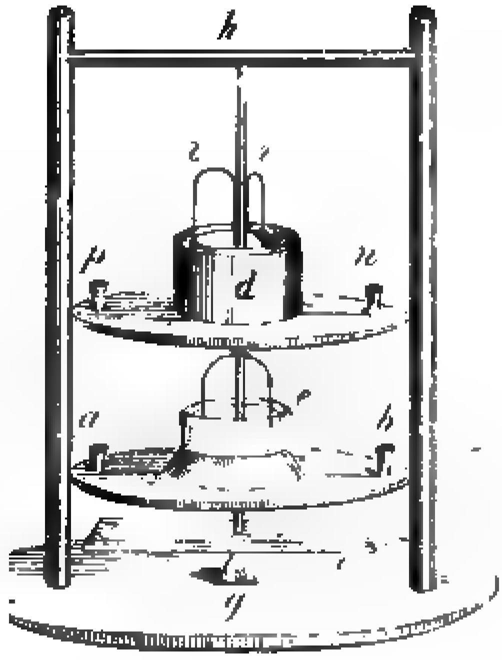




Fig 2




Alvey, Lith 128, London Rd